THURSDAY, MAY 27, 1886

A HAND-BOOK TO THE HISTORY OF PHILOSOPHY

A Hand-Book to the History of Philosophy. For the Use of Students. By Ernest Belfort Bax. Bohn's Philosophical Library. Pp. 405. (London: George Bell and Sons, 1886.)

THE task to which Mr. Bax has set himself in writing a short and at the same time intelligible account of the history of philosophy is anything but an easy one. The historian of philosophy finds himself in presence of an enormous amount of material, which has accumulated as system has followed system in what at first sight seems a bewildering succession. It will naturally occur to the reader to ask whether after all the history of superseded systems of philosophy is of much more than antiquarian interest, or whether at best its study can be expected to repay the necessary trouble.

There is a pretty widespread idea that philosophy, or "metaphysics," has led to nothing but disappointing failures in the past, and cannot from its very nature lead to any result of real value. This idea would appear to depend on a radical misconception as to the nature and scope of philosophical inquiry. Those in search of first principles in any department of science cannot fail to come across questions which they find cannot be solved by the methods which may be applied to the ordinary questions occurring within their science. Such questions occur naturally and necessarily in mathematics, physics, biology, art, &c., and in virtue of their similarity may be all classified as philosophical questions. As a familiar example we may take the question, which must necessarily come prominently before the physiologist, as to whether consciousness is a function of the body. It is pretty generally acknowledged that this question cannot be solved by experimental methods. The question is a philosophical one, and can only be attacked by the method of philosophy.

Let us see what this method is, and how it is to be applied to the case in question. Mr. Bax defines philosophy as the result of the endeavour to reconstruct the world according to its possibility. Applying the method here indicated we have to ask whether it is possible to conceive the world of our experience on the supposition that consciousness is a function of the body. This was substantially the question which Locke set himself to answer; and it was finally answered by Hume, who showed that the supposition in question led necessarily to its own annihilation. It remained for Kant and his successors to point the way to the only hypothesis consistent with the facts.

Such being the scope and method of philosophy, we may readily understand that its history is no mere record of an arbitrary series of speculations successively displacing one another, but never leading to any permanent result. Modern philosophy has centred round the discussion of the relation of matter and thought; and its successive systems form so many landmarks in the progress towards a solution of the fundamental questions involved in this discussion. Each system is doubtless more or less burdened with superfluities and errors of detail; and many philosophical works have been written by men famous in their day, but who failed to realise the true position of philosophical thought in their time, and thus cannot be assigned a permanent place in the history of philosophy.

Progress in philosophy is nevertheless just as well marked as in any department of science; and it is the special merit of Mr. Bax's hand-book that this progress is everywhere clearly brought into prominence.

There is one important respect in which the history of philosophy differs from that of any of the sciences, and which gives it a far greater relative importance. The detailed results of a science have a value of their own more or less independently of theoretical considerations or of other facts of the science in question. Thus the experimental results of chemistry have each a value independently of the truth of the vast majority of the other experimental results of the science, and even of the atomic theory itself. In philosophy, on the other hand, the conclusions arrived at are closely interdependent, and have no value apart from the general conception to which they belong, and the process by which that conception has been arrived at. It is as if the conclusions of chemistry were entirely valueless apart from the atomic theory and its correct application in detail. If this were so, it is evident that the history of the atomic theory and its application would be the first essential for the student of chemistry, instead of being what, as a matter of fact, many students of chemistry have only a very hazy notion of. Philosophical conclusions may be said to include the process by which they have been arrived at, so that a knowledge of the history of philosophy is in reality the basis of all study of philosophy. For this reason it will probably only lead to perplexity and disappointment to attempt the study of any philosopher without knowing the point at which he took up the work of his predecessors. Just as the individual organism shadows forth in its own development the forms assumed in the evolution of the stock to which it belongs, so the student of philosophy must repeat in his own mind the essential points in the historical development of philosophy.

A detailed criticism of Mr. Bax's work would scarcely be in place here. The book is on the whole an excellent piece of work. It is less of a summary, and much more readable, than the similar work of Schwegler, and for this reason will probably be preferred by English students. Due weight is as a rule given to the elements in any philosophical system which were of permanent value in influencing subsequent thought, while systems which were in reality anachronisms, however much stir they may have made, are passed over rapidly.

There are few positive blots in the book. One of these, cropping up in one form or other at various places, consists in the writer's persistent identification of the "anti-worldliness" of Christianity with "other-worldliness." As regards this and other kindred subjects the candour of Mr. Bax's expressions of opinion will, however, thoroughly commend themselves to the reader.

J. S. HALDANE

ELECTRICITY TREATED EXPERIMENTALLY

Electricity Treated Experimentally. By Linæus Cumming, M.A. (London: Rivingtons, 1886.)

THOSE who are acquainted with Mr. Cumming's "Introduction to the Theory of Electricity" will welcome most heartily a new and excellent little work from his pen. The book before us is on "Electricity Treated Experimentally"; and it is highly to be commended. It is admirably clear and concise, and at the same time the information is full and is well arranged; while the multitude of excellent illustrations and the open double-leaded type make the little book very pleasant and satisfactory reading.

The portions devoted to magnetic and electric measurements, both electro-static and electro-kinetic, are, as we should expect from the author, clear and full; while the descriptions of the various measuring instruments are very satisfactory. An excellent account is also given of Faraday's experimental investigations in electro-statics and electro-magnetism, and of those of Ampère in electro-dynamics.

The least satisfactory portion of the book is the chapter headed "Current Induction." This chapter, even making all allowances for its necessary brevity, requires very considerable improvement and amendment. The descriptions given of dynamo-electric machines are very far from adequate, even to the extent of making little or no distinction between a magneto-electric machine and a so-called "dynamo." Under the heading "Siemens Dynamo" there is a description and diagram of the old Siemens shuttle-wound armature; and Fig. 218, which is a diagram of a Gramme magneto, shows the soft iron of the armature cut away almost to nothing to make space for the armature. The information given with respect to the incandescent lamps and incandescent lighting also requires improvement to make it suitable for the present day; and the description of the telephone and of experiments to illustrate the action of it are not satisfactory. Some of these instruments it is perhaps unnecessary to treat of in a book of this class; but if they are dealt with at all the treatment must be correct and not too meagre.

One or two other minor matters we cannot avoid mentioning. The first is the naming of the magnetic poles. It is greatly to be desired that strong efforts should be made by all teachers to get rid of the English "north" and "south." Most writers of importance are doing this now; either by adopting "blue" and "red" for *true* north and *true* south respectively, or else by using in full the designations "true north" and "true south." However this may be, the practice of marking the ends of a magnet + and − seems to us thoroughly objectionable.

Next we would call the author's attention to the fact that the rule which he has called Oersted's rule for finding the direction in which a magnet turns under the influence of a current is commonly, and we believe rightly, called Ampère's rule. But it would be of very great advantage if Ampère's rule were improved out of existence, and some such rule substituted as that "terrestrial currents *supposed* to correspond with terrestrial magnetism follow the sun." When the unfortunate student imagines himself lying on his face, or (?) back, with a current entering by his feet, or (?) head, and stretches out his right hand, or (?) left, to show the direction of the deflection of the magnet, the probabilities against his coming at the end of his imagining to a correct conclusion are considerable. It seems strange that such a rule should have held its place from Ampère's time till now.

Lastly, we miss the name of Cavendish and his proof (by means of the experiments of Faraday so well described) of the electro-static law of the inverse square of the distance. It is impossible, by means of the torsion-balance, to give anything but a rough proof of this great law. But Cavendish established mathematically that no other law than that of the inverse square of the distance will account for the whole electric charge being found on the outside of a closed conductor; while the experiments of Faraday established to minute accuracy this celebrated law of electric distribution. In searching for the name of Cavendish, too, an alphabetical index would have been of much assistance. It is sad for a reviewer to take up a book without an index! No book, unless it be a novel, should be without one. For small books it is easily made; for large books it is essential.

With these criticisms we must take our leave of Mr. Cumming's book; but we cannot do so without remarking once more that it is one of the pleasantest and most thorough little books on electricity and magnetism with which we are acquainted. J. T. B.

OUR BOOK SHELF

Constructive Geometry of Plane Curves. With Numerous Examples. By T. H. Eagles, M.A. Pp. xx., 374. (London: Macmillan and Co., 1885.)

THIS book differs considerably from previous treatises on practical geometry. The author has made a serious attempt to improve the instruction usually given in his subject, and the result is that we have a text-book which will lend itself to class-teaching of a thorough and searching character.

Hitherto much time has been spent on constructions which furnish no mental discipline. In this treatise the proofs of the methods used are given or indicated in every case.

A valuable collection of examples is supplied at the end of each chapter. If a numerical result is involved, the answer is usually appended, and hints are given towards the solution of the more difficult examples.

Two-thirds of the book is devoted to conic sections, and herein we find methods of drawing these curves under almost any conceivable conditions; there are also chapters on reciprocal polars and the anharmonic properties of conics which will give the draughtsman some indication of the power of modern geometry and of its usefulness in practical application.

After a chapter on conics as derived from plane sections of a cone, we have about 100 pages devoted to various other curves which are of interest in mechanics or physics. Compared with the exhaustive treatment of the conic sections, the account of several of these curves is somewhat scanty.

We should like to see more space given to equipotential curves, for instance, and to have further exemplification of the methods of construction adopted by Rankine and Maxwell.

The book closes with an interesting chapter on the graphical solution of quadratic equations and certain trigonometrical equations.

Scientific Memoirs by Medical Officers of the Army of India. Edited by B. Simpson, M.D., Surgeon-General with the Government of India. (Part I., 1884). (1) On the relation of cholera to Schyzomycete organisms, by D. D. Cunningham. (2) On the presence of peculiar parasitic organisms in the tissue of a specimen of Delhi boil, by D. D. Cunningham. (Calcutta, 1885).

IN the first of these memoirs Dr. Cunningham makes some interesting additions to our knowledge of the presence and distribution of comma bacilli in the intestinal contents in cases of Asiatic cholera; on the occurrence of peculiar comma bacilli associated with the scum formed on tank water by Euglenæ; and on certain modifications in morphological and other characters in artificial cultivations of the choleraic comma bacilli.

The second memoir gives a minute description of the anatomical nature of the skin disease known as "Oriental sore" or "Delhi boil." This description is the more valuable as it is the first accurate account that we possess of the minute anatomy of this interesting malady. The value is enhanced by the discovery by Dr. Cunningham in the diseased tissue of a peculiar fungus bearing the characters of Mycetozoa or Myxomycetes, more especially of the subdivision of the Monadinæ; the distribution of this fungus is such that a causal relation of it to the disease process becomes highly probable.

The memoir is illustrated by numerous fine lithographs, many of them coloured. E. KLEIN

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Thomson Effect as Expounded by Prof. Tait

AMONG modern expositions of the subject of thermo-electricity there is none so full, and on the whole so instructive to students, as that contained in Prof. Tait's "Heat." It is therefore the more important to call attention to what appears to me to be, to say the least, a very questionable statement there made. It refers to the Thomson effect.

Thomson's experiments were of the following nature. A metallic bar was surrounded with a hot-water jacket in the middle and with cold-water jackets at the ends, and there were two holes sunk in it for the insertion of thermometers, midway between the hot jacket and the two cold jackets. When the flow of heat had become nearly steady, a steady current of electricity was sent through the bar; and, after it had flowed for several minutes in one direction, it was reversed; then, after the same number of minutes, it was again reversed, and so on several times. It was thus found that, when the bar was of copper, the current made the temperature of the further thermometer higher than that of the near one (the words *far* and *near* being used with reference to the end at which the current entered). When the bar was of iron, the current made the temperature of the near thermometer higher than that of the further one.

Seeing that a current may be regarded at pleasure as the flow of vitreous electricity in the nominal direction of the current, or as the flow of resinous electricity in the opposite direction, Thomson summed up his results by saying that "the vitreous electricity carries heat with it" in copper, and "the resinous electricity carries heat with it" in iron. He also gave the name of "electric convection of heat" to the effect thus detected. It has since been called by others "the Thomson effect."

The experiments were instituted to test the truth of a conclusion of which he had previously given a theoretical proof—the conclusion that "in one or other of the metals, and most probably in both, there must be a thermal effect due to the passage of electricity through a non-uniformly heated portion of it, which must be an absorption of heat [a cooling] or an evolution of heat [a warming], according to the direction of the current between the hot and cold parts."

It may be taken to be an established fact that, in a uniform linear conductor along which a current is flowing, there is, in addition to the frictional heating, which is proportional to the square of the current, a warming or cooling effect proportional (at given temperature) to the steepness of the thermometric gradient at the point which is warmed or cooled, changing sign with the gradient, and vanishing at points of maximum or minimum temperature, where the gradient vanishes.

Now compare these effects with what happens when a stream of liquid flows through a pipe surrounded at alternate points in its length with hot and cold jackets, the average temperature of the water being the same as the average temperature of the pipe. It will carry heat from the hotter to the colder portions, thus cooling the hottest parts, warming the coldest parts, and at the same time carrying forward the points of maximum and minimum temperature. If, at each point of the pipe (supposed straight and horizontal), we erect an ordinate to represent its temperature, and call the curve of which they are the ordinates "the temperature curve," the effect of the flow of liquid on this curve will be twofold: (1) it will carry the temperature curve forward; (2) it will make the temperature curve flatter.

Thomson's experiments show that an electric current carries the temperature curve forward in copper, and backward in iron; but I am not aware of any evidence to show that it makes the temperature curve flatter.

The analogy between the Thomson effect and convection of heat by a liquid in a pipe therefore does not run on all fours, and must be used with caution.

Maxwell says ("Elec. and Mag.," p. 343, second edition), "positive electricity in copper, and negative electricity in iron, carry heat with them from hot to cold." The words "from hot to cold" are here added to Thomson's original phrase "carries heat with it," and the addition thus made is not in accordance with facts, for it implies that heat is taken away from the hot parts and given to the cold parts; whereas the fact is that heat is taken from parts where the temperature gradient is in one direction, and heat is given to parts where the gradient is in the opposite direction. If the statement be altered by a little transposition, so as to make it stand thus, "positive electricity in copper, and negative electricity in iron, going from hot to cold, carry heat with them," it will be scarcely distinguishable from Thomson's original statement.

Prof. Tait goes further, and says ("Heat," p. 170):—"After a series of elaborate experiments (described in the *Phil. Trans.* for 1855) [it should be 1856] Thomson found that:—

"*An electric current in an unequally heated copper conductor behaves as a real fluid would do*, i.e. *it tends to reduce differences of temperature. In iron it tends to exaggerate them.*"

The italics are Prof. Tait's.

I can find nothing in Thomson's paper to support the assertion that in copper an electric current tends to reduce differences of temperature, though the idea that it does so is naturally suggested by the analogy implied in the phrase "electrical convection of heat."

The statement that in iron the current tends to exaggerate differences of temperature, seems to be completely original on the part of Prof. Tait. It does not arise naturally out of Thomson's dictum, "resinous electricity carries heat with it in iron"; for if we think of resinous electricity as a real fluid flowing through iron, it would tend to equalise differences of temperature in that metal.

The two statements taken together suggest the following line of reasoning as conclusive against them both:—

Let there be the same initial distribution of temperature in a copper and in an iron bar, and currents in the same direction through both. Then the alterations of temperature at corresponding points in the two bars will have opposite signs. Any one who maintains that the warmest parts of the copper are cooled is therefore bound to maintain that the warmest parts of the iron are warmed. But there is precisely the same ground for maintaining that the warmest parts of the iron are cooled, and therefore the warmest parts of the copper warmed. Whatever vitreous electicity can do in copper, resinous electricity can do in iron. We are thus involved in a contradiction if we assume any finite heating or cooling at the hottest parts. And similar reasoning disproves any finite heating or cooling at the coldest parts.

The following formal investigation confirms the view which I have above expressed.

Leaving out of account frictional generation of heat (in other words, the effect which varies as the square of the current), let $\sigma d\theta$ be the heat generated in unit time by unit current in a uniform copper bar, in passing from a section where the temperature is $\theta + d\theta$ to one where it is θ (see Thomson's "Papers," vol. i. p. 246). Let x be distance along the bar in the direction of the current, and c the thermal capacity of unit length of the bar. The heat generated in a short length δx is $-\sigma\frac{d\theta}{dx}\delta x$, and the consequent rise of temperature in this portion is $-\frac{\sigma}{c}\frac{d\theta}{dx}$. This is in unit time. Hence, putting t for time, and v for $\frac{\sigma}{c}$, we have—

$$\frac{d\theta}{dt} = -v\frac{d\theta}{dx}.$$

If the limits of temperature are not very far apart, it is known that σ and c are sensibly independent of θ; hence v may be treated as a constant. The integral of the above equation is then—

$$\theta = F(x - vt),$$

where F is a functional symbol such that $\theta = F(x)$ expresses the original distribution of temperature. The interpretation is that the original "temperature curve" travels forward with velocity v without flattening or any other change of form.

Belfast, May 14 J. D. EVERETT

Scientific Nomenclature

SOME time ago Mr. "John O'Toole," in the columns of NATURE, waged war against that hideous monstrosity *Potential Energy*, and he very aptly summarised his case against this term by saying that it involves, by the very signification of words, "a double remotion from actuality."

A few months ago it occurred to me that to express what is intended by the potential energy of any system the term *Static Energy* is not only logically unobjectionable but specially fit for the purpose. Thus, if a string or a membrane is stretched, a wire bent and twisted, or, generally, a body strained in any manner, the work which it can do against resistance in returning from its state of strain to its unstrained condition is the *Static Energy* of the system in the strained condition. The work which a moving system can do in virtue solely of its motion is, of course, its *Kinetic Energy*.

Thus we have simply *Static Energy* and *Kinetic Energy*, and these terms have the further advantage of harmonising with the ordinary subdivisions of dynamics.

I may add that the term *Static Energy* has received the approval of all the mathematical physicists to whom I have submitted it. GEORGE M. MINCHIN

R.I.E. College, Cooper's Hill, May 19

Pendulum Oscillation

THE oscillations of a long pendulum are observed to describe an ellipse the axis of which tends to set itself at right-angles to the plane in which the pendulum was started. An explanation of the above phenomenon would much oblige. M. H. MAW

Walk House, Barrow-on-Humber, Hull, May 18

What is Histioderma?

Histioderma hibernica was described by Dr. Kinahan in 1858 as an annelid, and most writers who have since mentioned it have also regarded it as such. Dr. Haughton notices and figures it in his "Manual of Geology," Sir R. I. Murchison places it amongst annelids in his "Siluria," as also Dr. Bigsby in his "Thesaurus Siluricus." Writing from memory only, I think a description and figure will be found in W. H. Baily's useful compendium of Palæozoic British fossils, published a few years ago. The fossil is mentioned with more or less detail by numerous writers on Cambrian strata.

The above references will perhaps answer the letter signed "S.," but there still remains the question, What is Histioderma? It is apparently a fossil impression in the rocks of Bray Head, Wicklow, Ireland. Only one species and only one locality is, I believe, known. It seems to me very doubtful what kind of animal made the impression—whether an annelid, or otherwise. I am inclined to doubt if the rocks are Lower Cambrian, as geologists generally suppose, because the evidence seems to indicate that the Bray Head rocks had become hardened and raised into land before the oldest Lower Cambrians of Wales and England were deposited. I would therefore pass on the question and ask, What is Histioderma?

4, Cowper Road, Acton, London, W. A. RAMSAY

IN answer to the above question in your issue of May 20 (p. 53), I refer your correspondent to the *Proceedings* of the Geological Society of Dublin, where he will find (*Natural History Review*, vol. v., *Proceedings* of Society, p. 150) the original description of Histioderma by Prof. Kinahan. According to its author, Histioderma is the tube of a cephalo-branchiate annelid. J. VICTOR CARUS

Leipzig, May 23

Black Skin

IN a letter lately received from Mr. Flinders Petrie, who is now in Egypt, are some remarks on "Black Skin," which I think very interesting, and perhaps the readers of NATURE may think them so too, so I send the paragraph to you.

Belvedere, Kent, May 24 F. C. J. SPURRELL

"In considering the use of a naturally or artificially black skin, we should not look so much to the requirements of the surface, which is constructed to bear variations, and has the means of cooling and maintaining a proper temperature within itself, but rather we should consider the far more delicate tissues beneath. We all know how translucent flesh is to strong light, and it can hardly be doubted that the rays of a tropical sun would light up a white man's inside considerably; whereas black skin would stop out the solar energy of light, heat, and chemical rays effectually. Skin heat is of no importance, as perspiration can always keep that down. May not the oiling of the skin in hot countries be partly to make it reflective, so that it should absorb less heat? And may not the regard white races have for clothing be partly for the purpose of keeping the insides of their bodies sufficiently in the dark?"

Male Animals and their Progeny

CAN any of your correspondents inform me whether any of the male wild animals in foreign countries show any love for, or recognition of, their progeny? In this country amongst the domestic animals it does not seem to exist, save in the case of the gander, who carefully guards the goose while sitting, and attends to the goslings when hatched; but the cock pays no attention to the hen while sitting nor when with chickens, nor does the dog, the bull, the horse, nor the boar evince any sign of parental feeling under circumstances favourable to its development.

One peculiarity of geese is, I think, worthy of notice, for it is not possessed by ducks or fowls, who also live in flocks. If when goslings are hatched, they are permitted to run with a goose in company with the other geese of the flock, all chances of any eggs being laid by the other geese who have no goslings are over. P.

Birds and Mirrors

FOR six days I have been for two or three hours in the club reading-room, where t ere is a large mirror. During all this time a cock and hen sparrow have been flirting with and bowing to their images in the glass with evident pleasure—rushing along the mantel-shelf, flying to the top of the frame, or resting for a time, always apparently happy in contemplation, never showing disappointment. F. C. CONSTABLE

Sind Club, Karachi, May 2

SURGEON-MAJOR T. R. LEWIS

DR. TIMOTHY LEWIS, Surgeon-Major Army Medical Staff and Assistant Professor of Pathology in the Netley Army Medical College, whose death, which took place on May 7, we announced last week, was a native of South Wales, and received his medical education at University College, London, and graduated as M.B. at Burlington Gardens. He was selected about twenty

years ago, together with Dr. Cunningham, to proceed to India in order to undertake, under the auspices of the Indian Government, a prolonged study of the causation of cholera, especially in regard to its reputed relation to parasitic organisms. At that time little was known or thought about Bacteria, and the public mind had been aroused by Hallier's (now long-exploded) theory of a rice-fungus as the cause of cholera, just as more recently it has responded to Dr. Koch's invitation to believe in the comma-bacillus. Dr. Lewis and his companion were authorised to visit Prof. Hallier at Jena and Prof. de Bary (in those days not attached to French Strasburg!) for the purpose of acquainting themselves with methods of mycetological research before proceeding to India. A few weeks was all the time allowed them for this visit, and consequently they took little to India excepting their own conscientious habits of work and that modicum of knowledge of microscopic *technique* which was considered sufficient for the highest medical qualification in England in those days. Nevertheless these observers made most valuable and minute researches on the microscopic organisms present in the dejecta of cholera-patients, which were published by the Government of India. Dr. Lewis extended his researches into the general question of the presence of microscopic organisms in the blood and tissues of man in health and disease, and was led to some very interesting discoveries. His results were published from time to time by the Government of India, and were re-published as they appeared in the *Quarterly Journal of Microscopical Science.*

Dr. Lewis's work was always remarkable for the extreme care with which positive results were asserted, and for the fairness with which the researches of predecessors in the same field were considered and discussed. His most remarkable discovery was that of the little nematoid worm occurring in the blood of persons suffering from a form of chyluria and elephantiasis, to which he gave the name *Filaria sanguinis hominis.* This discovery was published in 1872. Some years later Dr. Bancroft discovered in Australia the adult worm from which the brood of minute blood-parasites is derived, and, still later, an unsuccessful attempt has been made by Dr. Patrick Manson to show that the young pass an intermediate stage of existence in the alimentary canal of gnats, which suck them in together with the blood of worm-infested persons.

It is no small thing in these latter days to discover a new human parasitic worm of great pathological significance, and it was in recognition of this discovery, as well as in view of his important contribution to the discussion of "the cholera-bacillus theory," that the Council of the Royal Society in last April selected Dr. Timothy Lewis as one of the fifteen candidates to be submitted to the Society for election in next June.

In regard to the question of the relation of Bacteria to cholera and similar diseases, Dr. Lewis had a vast store of both published and unpublished observation. With characteristic caution and modesty, he had refrained from dogmatising on the subject. Working for twelve years in Calcutta, with daily access to cholera patients, he was thoroughly familiar with the several different forms of Bacteria which are to be found in the alimentary tract and in the dejecta of choleraics. Unlike some of his recent successors in this line of research, Dr. Lewis was also familiar with the different forms of Bacteria which occur in the *healthy* human mouth and intestines, and in potable waters. He was therefore able to demonstrate immediately on the publication of Koch's figures of the so-called "comma-bacillus" that this *form* (asserted by Koch to be peculiar to cholera evacuations) was nothing more nor less than a *Spirillum* broken by manipulation, and commonly to be found in the mouth of healthy persons. The importance of this contribution to the controversy excited by Dr. Koch's statements cannot be too highly estimated. Its accuracy was universally recognised at once, and has never been called in question. Dr. Klein has since come to the conclusion that not only are organisms of the exact *form* of Koch's cholera-comma abundant in the healthy saliva, as shown by Lewis, but that some of these forms have precisely the same physiological conditions of growth, and precisely the same action upon gelatine as Koch considered to be characteristic of those obtained from cholera evacuations.

At the time of his death Dr. Lewis was carrying on in his laboratory at Netley an extensive series of culture and inoculation experiments, chiefly upon the Bacteria which occur in the alimentary canal of man.

Those who enjoyed his personal friendship valued Dr. Lewis for his warm-heartedness no less than for the rare combination of enthusiasm with caution in his work which gives his published results a very special value. It is perhaps some satisfaction to his friends to know that he had heard of the recognition of his merits by the Royal Society Council before the commencement of the attack of inflammation of the lungs which so rapidly ran to a fatal termination. E. R. L.

A SKETCH OF THE FLORA OF SOUTH AFRICA

UNDER this title we would draw the attention of botanists to a very able essay on the botanical regions of South Africa, contributed to the "Official Hand-Book of the Cape of Good Hope" for 1886, by Harry Bolus, F.L.S., an accomplished botanist, who has devoted many years to the investigation of South African plants.

That extra-tropical South Africa is one of the most varied botanical regions on the globe is a fact familiar to both botanists and gardeners, from the days of Linnæus,—who epitomised its richness in the expression, "Ex Africâ semper aliquid novi,"—and of the earliest cultivators of greenhouse plants, who were indebted to the Cape of Good Hope, far more than to any other regions of the globe, for what were, and till Japan and Australia eclipsed it, the prime favourites of the conservatory. There are those still alive who can remember the time when plant-houses were ornamented with little else than Cape Heaths, Pelargoniums (miscalled Geraniums), Polygalas, Proteas, Oxalis', Mesembryanthemums, Everlastings, Stapelias, Irideæ, and Cape bulbs innumerable, and when the illustrated horticultural serials of the day were either devoted to these, or contained figures of more of the plants of this than of any one other country. It is true that the cultivation of all but a very few of the heaths and geraniums has been abandoned for things of not greater beauty, and of far less interest, but this is due, not to want of appreciation, but in the case of some to their not being amenable to the treatment of the "soft-wooded" plants now in vogue, and of others to the fact that their flowering period—of the bulbs especially—is a very brief one, and that the flowers soon fade when cut.

To return to the little essay before us: the attempt to define the South African regions of vegetation is not a new one; it had been essayed by Meyer and Drège, Zeyher, Griesebach, and others, but not successfully; and the author of the sketch under consideration is the first who has succeeded in presenting satisfactorily the salient botanical characters of that flora, as affected by, or in correspondence with, geographical and other physical conditions; whilst he alone has given such vivid pictures of the vegetation of the different botanical regions he has defined, that any one with even an elementary knowledge of South African plants can fancy himself travelling over the ground.

The two dominant features of the South African flora are, the number of orders, genera, and species that it contains, and the limitation of great groups of these

within very narrow and well-defined areas. There are five of these areas, differing from one another in the aspect as well as in the composition of their floras more decisively than do any other five contiguous areas of similarly small extent on the surface of the globe. These five together have been estimated to contain the extraordinary number of 14,000 species of flowering plants! which are comprised under 200 natural orders (nearly three-fourths of the known orders of plants), and 1255 genera (one-sixth of the described genera of the whole world). Though possessing no truly Alpine region, it is by far the richest extra-tropical area on the globe in respect of genera and species, and is probably not surpassed by any tropical area of equal extent; a circumstance which may be taken together with the fact, that the vast proportion of the species are low herbs or small shrubs—trees being very rare both in species and individuals, and that there is not a single arboreous genus of more than a very few species. There is no dominant genus of trees like the Eucalypti of Australia and the Conifers of northern regions, or even the oaks or beeches of Europe, that monopolise great areas and determine the absence or presence of a multitude of plants of lower stature. The following are Mr. Bolus's regions:—

(1) *The South-Western Region.*—This (which might be called the Cape proper region) extends in a curve from near the mouth of Olifant's River along the coast facing the Western Atlantic, round by the Cape of Good Hope to Cape Agulhas, and thence to near Port Elizabeth along the coast facing the Southern Atlantic. Throughout its length—about 600 miles—it maintains a breadth of between 40 and 80 miles, never more or less, and is bounded on its landward sides by mountain-ranges attaining 4000 to nearly 8000 feet in elevation—of which the eastern run east and west, the western north and south. The surface is varied with bushy, grassy, sandy, and rocky tracts, of which some appear desolate from a distance, but on examination are found to swarm with genera and species. It is a region of small-leaved herbs and bushes—of Irideæ, Orchideæ, Rutaceæ, Ericeæ, Restiaceæ, Compositæ, Proteaceæ, Polygaleæ, Mesembryanthemums, Oxalideæ, Geraniaceæ, and Leguminosæ. It is that whence all the Cape plants of the greenhouses of the last generation were derived. The climate is dry, temperate, and comparatively equable, with a winter rainfall which varies excessively from 24 inches at Cape Town to 60 in some of its own suburbs, but everywhere rapidly diminishing with distance from the coast and from the vicinity of Cape Town. The few forests are near the few rivers, and their trees rarely exceed 50 feet in height. As an instance of the endemic nature of its vegetation, the genus Erica forms one of the many conspicuous examples. It contains no fewer than 300 Cape species; all, or nearly all, are confined to this region, and various other genera contain upwards of 100 endemic species. The total number of flowering plants in this region is about 4500. No temperate area of the globe of its extent is nearly so peculiar or rich. California offers but a faint counterpart; and the restriction of the majority of the species of Cistus and Ulex to the Atlantic coasts of Europe offers an even fainter example of restricted distribution.

(2) *The Tropical African Region* (which might be called the Natal region).—Unlike the western temperate coast, the vegetation of the eastern temperate retains the characteristic features of that of tropical Africa. From Port Elizabeth northwards to Abyssinia there is no sharp delimitation of floras. This region is bounded on the east and south-east by the Atlantic, and stretches inland for from 60 to 100 miles to ranges of mountains of 5000 to 10,000 feet high, which bound it on the west. The surface is varied with bush and park, which, proceeding northward, give place here and there to extensive forests, and it is traversed by many streams. The herbage, and bush- and tree-foliage, are greener than in the south-western district, and the foliage larger. The rains are summer ones, the temperature rather higher than in the west, and much more so to the north. Though there is some overlapping of the vegetations of this and the Cape region proper in the neighbourhood of Port Elizabeth and Grahamstown, the transition from one to the other is wonderfully sudden. The 300 heaths may be said to disappear bodily, as do the Cape Rutaceæ, Proteaceæ, and Orchideæ. As suddenly appear giant Cycadeæ, Aloes, leafless succulent tree Euphorbias, with different tribes of Orchideæ, Leguminosæ, and Amaryllideæ, often grouped in striking assemblages of grotesque forms, whilst a palm, *Phœnix reclinata*, reaches lat. 33° S. Along with African types, Indian abound, both in genera and species, especially to the northward.

(3) *The Karroo Region.*—Returning now to the Western Atlantic coast of South Africa, from Olifant's River to the Orange River, and thence south-eastwards, bounding the Cape proper region on the north, extends a vast shallow basin about 2000 feet above the sea-level, except towards its western or littoral boundary. It forms a curve somewhat concentric with that of the Cape region proper, and extends a little further east into the tropical African region. It is about 700 miles in length, and from 30 to 70 in breadth from the bounding mountains on the north, which are the Nieuveld Bergen, to lower ranges on the south. The surface consists of sandy, stony, or fertile plains of vast extent, traversed by river-beds, and by the courses of torrents filled by summer thunderstorms, but dry in winter. Permanent water is scarce, and (as in California) sheep have denuded large areas of native vegetation. The climate is excessive; the rainfall, chiefly a summer one, from 7 to 14 inches according to locality. During the dry season the country is a desert, but after a shower it is suddenly, but transiently, transformed into a vividly-coloured garden.

"I was amazed on visiting that desert country, after the rains of June to July 1883, to see tracts, hundreds of acres in extent, covered with sheets of living fire or glowing purple, visible from several miles' distance, caused by the beautiful Compositæ in flower; and nothing is more curious than to see this luxuriance intermingled with the black or white branches of dead shrubs killed by previous droughts, standing like ghostly intruders on a scene of merriment and joy. These charming scenes pass away all too rapidly, and in a month or two little that is beautiful remains."

The only tree is the ghastly *Acacia horrida*, fringing the dry river-beds. Of the Orders of the Cape region proper, Ericeæ, Restiaceæ, Polygaleæ, Orchideæ, Proteaceæ, Rutaceæ, almost all are absent, and a variety of so-called succulent plants appear, especially innumerable species of Mesembryanthemum, together with Portulaceæ, Zygophylleæ, Crassulaceæ, Stapelias, Ficoideæ, and dwarf Euphorbias, besides which many other genera that are never succulent elsewhere, here have species with fleshy roots, stems, or leaves. The tree Aloes of the Eastern region are fairly well represented, but by different species, and the same remark applies to the Geraniaceæ of the Cape region. The curious Elephant's-foot (Testudinaria) is a characteristic plant, as are the two species of the parasitical Rafflesiaceous genus Hydnora, which extends to Abyssinia.

(4) A very singular region or subdivision of the Karroo region is called by Mr. Bolus the "Upper Karroo," or "*region of Compositæ*," which occupies an inland broad area north of the Karroo region, everywhere distant from the coast, about 400 miles from east to west, and 150 to 200 miles in breadth. It is a mountainous country, only partially explored, and supposed to have a mean elevation of 4000 to 5000 feet. Its prevalent features are that of a treeless, heathy tract, or dry elevated moorland, covered with shrublets of a dull hue. The climate is severe, the summer nights are always cool, sharp frosts are common,

and snows falls in winter. Compositæ abound. Of nearly 1000 species that have been collected in this region, 61 genera and 231 species are referable to this Order. Of Orchids only four species have been found; Rutaceæ, Ericaceæ, and Restiaceæ are all but absent, and Proteaceæ wholly so.

(5) *The Kalahari Region* is the name Mr. Bolus gives to the vast tract of country north of the Composite region, west of the Natal region, and south of the tropic of Capricorn. It is a desert country, with an extreme climate, a rainfall of summer thunderstorms, hot summers with cool nights, and frosts in winter. Its essential features are of a country clothed with grass in tufts, and isolated shrubs and trees, which form forests in the north, and are thus probably continuous with the forests of tropical Africa. The Cape flora has here disappeared, and with it we take leave of Mr. Bolus's very able and most interesting contribution to botanical geography, regretting that want of space prevents any notice being taken of the many valuable observations and comparisons that he has made relating to the affinities of the South African with the Australian flora, and other matters of scientific interest. J. D. H.

THE INTERNATIONAL COMMITTEE OF WEIGHTS AND MEASURES

THE Comité International des Poids et Mesures, which has its bureau at Sèvres, has recently issued its ninth Report to the contracting Governments. The Report gives an account of the work done by the Committee during 1885, and a statement of the probable expenses of the Committee for the current year. During the last year new instruments for the accurate comparison of standards of the metric system have been obtained at a cost of about 500*l.*, making a total cost of about 7000*l.* for instruments supplied to the Bureau. For the present year the expenses of the Bureau are estimated at 100,000 francs (4000*l.*), the annual expenditure of the Committee being limited by the Convention to that amount, of which sum about 2650*l.* is for payments to various officers of the Comité. These expenses are divided amongst the twenty contracting Governments, the annual contribution of Great Britain and Ireland amounting to about 300*l.* The new instruments include a comparator for measures of length by M. Brunner; new mercurial thermometers by M. Tonnelot; an air thermometer by M. Golaz; a spherometer by M. Brunner; and other measuring instruments by MM. Oertling, Boudin, Alvergniat, Simmen, and the Société Genevoise.

In the fourth volume of the *Travaux et Mémoires* of the Bureau (Paris, Gauthier-Villars, 1885) reference was made by the Director of the Bureau to the work then in progress, and in the present Report a summary is given of the whole work done by the Committee during the past twelve months.

The work of the Director has specially included the verification of the lengths and coefficients of expansion of several standard metres, and the determination of the weights and specific gravities of several standard kilogrammes for different Governments and scientific authorities. A report is given on the comparison of the new kilogramme prototype (K. III.) with the old kilogramme des Archives, and also on the verification at Paris, by Dr. J. Broch and Mr. H. J. Chaney, of certain British standards. During the present year the Committee report that Dr. Broch will continue his researches on the influence of light on the defining-lines of standards of length, and M. Benoit will also carry on his experiments on the best means of comparing end-measures of length (*mesures à bouts*) with line-measures (*mesures à traits*). Dr. Thiesen will continue his interesting studies on balances and the verification of kilogramme standards. Dr. Chappuis, with the assistance of Dr. Guillaume, will also continue the experiments on the verification of thermometers.

It would appear that the Committee are carrying out the duties intrusted to them with all possible care and despatch. In the preparation of the alloy of platinum-iridium, of which the standards of the kilogramme and metre are made, extraordinary difficulties were originally encountered, owing mainly to the presence in the iridium of iron, rhodium, ruthenium, osmium, and other metals, and the Committee therefore obtained the assistance of MM. Stas and Debray. The report of these eminent metallurgists shows that the Committee are indebted to Mr. G. Matthey for the production of an alloy having the high purity and finish required for such work.

The Report of the Committee includes copies of its correspondence with the different contracting Governments. In an able note to the Japanese Minister, Dr. A. Hirsch (the Secretary of the International Committee) explains the objects of the Metric Bureau, and it is gratifying to find that Japan has now joined the Convention. From the correspondence with this country it would appear that our Government will in due course obtain copies of the new metric standards.

The Committee announce that the vacancies caused by the death of Prof. Heer and the resignations of Gen. Wrede and Dr. Gould have been filled by the election by the Committee of Prof. Th. v. Oppolzer, Mr. W. H. M. Christie, and Prof. Thalèn.

The Report also includes copies of a correspondence with reference to a proposal made by the French Government to extend the functions of the Bureau to the determination and verification of electrical standards of resistance and light, for the use of the different Governments. The proposal appears to have gone no farther at present than asking the several contracting Governments whether they would have objection to the preparation of estimates showing the cost of a new or extended Bureau for electrical purposes. The replies of Belgium, Denmark, Spain, the United States, Italy, Servia, and Switzerland are attached, but, excepting Spain and Switzerland, it cannot be said that generally the Governments appear to have given the proposal the most cordial welcome.

THE WEATHER OF THE ICE SAINTS' FESTIVALS OF 1886

LAST year we chronicled (NATURE, vol. xxxii. p. 62) one of the most disastrous snowstorms that ever occurred at this season in Vienna, where, on May 15, there fell 5½ inches of snow, and the cold accompanying the storm was so intense that several persons who were exposed to it were frozen to death. Over Austria and Hungary snow covered the fields and vineyards, and, the crops being in a somewhat advanced condition at the time, an incalculable amount of damage was done. But the festivals of the Ice Saints (May 11, 12, and 13) this year have been marked off for a wider and deeper remembrance by storms of wind, rain, hail, and snow in all the continents of the northern hemisphere, which, for number and destructiveness, are perhaps unexampled at this time of the year.

In the British Islands the cold acquired its greatest intensity on the five days from May 11 to 15, and was coincident with a system of pressures which appeared in the Channel, and thence proceeded in a north-north-easterly direction through the North Sea to the south-west of Norway, which was reached on the 15th. It necessarily resulted from this distribution of pressure that northerly and easterly winds prevailed in these islands, and temperature fell correspondingly low. Over that

large portion of the British Islands lying between lat. 52° and 58°, the mean temperature of the five days was at least 6°·0 under the average of the season, and in the central portion of this district, as at York and Barrow-in-Furness, it was 8°·3 under the average. On the other hand, in Jersey the defect from the mean was only 1°·5, and in Shetland 2°·5. At Barrow-in-Furness, on the 12th, the maximum temperature was only 39°·0. On this day snow fell over the higher districts of central and southern Scotland, in many places to the depth of several inches, and the cold was so intense that swallows and some other birds perished in thousands.

Meanwhile torrents of rain were being poured down continuously over wide districts of northern and central England. These rains were heaviest and most widespread on the 11th, 12th, and 13th, on each of these days upwards of an inch being recorded in many places. Mr. Scott states in the *Weekly Weather Report* that during these three days falls of 4·83 inches occurred at Churchstoke, Montgomeryshire; 4·36 inches at Hereford; 4·15 inches at Pershore, Worcestershire; 3·38 inches at Ross, Herefordshire; and 3·44 inches at Fassaroe, county Wicklow. Extensive and disastrous floods were the consequence. The railway traffic between Worcester, Hereford, and Malvern was suspended, and the whole of the united valley of the Severn and Teme, in the neighbourhood of Worcester, was laid under water. At Gloucester large numbers of the inhabitants were driven from their homes. The valley of the Trent presented the appearance of a vast lake, and the Midland Railway between Nottingham and Derby was submerged. Immense damage was done about Chester and along the estuary of the Dee, over the low grounds bordering the Humber, and over extensive tracts of Yorkshire.

A noteworthy feature of the storm is shown in the weather maps for the morning of the 13th, when the area of low pressure extending from west to east over the south of England was broken up, and showed three satellite cyclones with still lower pressures, having their centres the first to the west of Brest, the second over Somersetshire, and the third over Kent,—these all evidencing great, well marked, and sharp local differences in the distribution of temperature and vapour. Further, a most remarkable satellite cyclone was seen near Perpignan, in the south of France, which in all probability represented the small cyclone or tornado that about seven o'clock of the previous evening wrought such dreadful havoc in Madrid, causing 32 deaths, seriously wounding large numbers, variously estimated from 320 to 520 persons, and wrecking hundreds of houses. The area swept over by the tornado was comparatively limited, but within that area, as in the lower Carabanchel, not a house was left undamaged and many were wholly blown down, and hardly any of the inhabitants escaped uninjured. The storm was preceded by a sultry atmosphere, a heavy bank of black clouds in the north, and torrents of rain, and as so frequently occurs with the tornadoes of America, many buildings would appear to have fallen to pieces from an explosive force from within as the tornado passed overhead.

On the 14th violent cyclones or tornadoes occurred at Krossen, near Frankfort on the Oder, at Linz on the Danube, and at Lonato, near Brescia, wrecking houses, and causing great destruction in other ways; and storms of less, but yet of considerable severity are reported from other parts of the Continent. It is to be hoped that some meteorologist of mark will undertake the discussion of these remarkable storms, so as to lead, if possible, to some knowledge of the peculiar meteorological conditions of Europe out of which they originated. In this discussion no little help will be given by the high-level observatories now established at so many points over Europe.

In the United States on May 11 a tornado passed over Kansas City, Missouri, destroyed the court-house and other buildings, and partly demolished the bridge over the Missouri River, and thence pursued an easterly course to Pennsylvania, killing in its course about 90 persons, and injuring 300. On the night of the 12th terrible destruction was done by tornadoes in Ohio, Indiana, and Illinois. From 4 to 5 inches of rain fell within three hours, fully twenty miles of the Little Miami Railway were washed away, and at Xenia, where the waterspout was most destructive, 36 persons were drowned, 80 injured, and more than 100 houses destroyed. On the 14th another series of tornadoes carried destruction through Ohio and Indiana; and on the 15th a tornado, which appears to have formed on Lake Erie, penetrated 100 miles into Ohio. Its path was about 500 yards wide, and it levelled everything with the ground; killed 41 persons, and injured several hundreds. These, with other tornadoes of less severity occurring in Kansas and elsewhere, are stated to be the most severe and destructive storms or tornadoes hitherto experienced in the United States, and the losses are estimated at about 5,000,000 dollars.

The detailed reports of these remarkable storms will no doubt be prepared and circulated with the fulness and satisfactoriness which characterise the work and publications of the United States Signal Office; and, considering the striking simultaneousness of occurrence of these American and European storms from May 11 to 15, Gen. Hazen would, if the Reports were accompanied and illustrated by international weather maps of the northern hemisphere from May 7 to 16, lay meteorologists under a deep obligation.

CRETACEOUS METAMORPHIC ROCKS

IT is now a good many years since Prof. J. D. Whitney announced the existence of Cretaceous sediments in California which had undergone metamorphism into various crystalline rocks. The attention of geologists is once more directed to that region by the recent work of the Geological Survey of the United States. Mr. George F. Becker, whose admirable monograph on the great Comstock Lode has reflected such lustre on that Survey, has more recently been intrusted with the investigation of the quicksilver deposits of California. This research when completed will form the subject of another monograph in the same great series of publications. But in the meanwhile some results of such startling importance in metamorphism have been obtained that Mr. Becker has published in the *American Journal of Science* a brief preliminary outline of them. It appears that the inquiry into the nature and origin of the rocks in question has been carried on partly by examination of their structure and relations in the field, partly by chemical analysis and partly by the study of microscopic sections. In short, all the appliances of modern geology have been enlisted in the investigation.

The area embraced by the rocks which are stated to have undergone metamorphism is estimated at 3000 square miles. The rocks are determined by fossils to be approximately of Neocomian age. They consist chiefly of sandstone and arkose lying upon and probably for the most part derived from granite. Their quartz-grains are cemented in great measure with carbonate of lime, and there are likewise clastic fragments of orthoclase, plagioclase, biotite, hornblende, and other minerals of the granite. From ordinary unaltered fossiliferous sandstones gradations are traceable into varieties wherein a process of recrystallisation has been set up, but has ceased before the fragmental character has been wholly effaced. In this process one of the first stages is often the resolution of the clastic grains into crystalline aggregates from which new minerals are built up. Thus the quartz-grains have had their surfaces so altered that an envelope of

twinned felspar microliths has formed round them. These bodies lie approximately normal to the surface of the residual kernel, which they actually penetrate like pins set in a cushion. Zoisite also, which is present in nearly all the altered sandstones, as a product of metamorphism, sometimes pierces the quartz from outside. Augite and hornblende have likewise been developed, not infrequently as microliths, which, though grouped together in one common crystallographic outline, are not united.

Further stages of change are described as occurring in certain rocks where the fragmental character, though to the unaided eye still traceable, is found to have been replaced by an entirely crystalline structure, giving rise to rocks which must be classed with the diabases. These masses sometimes have their pyroxene in the form of diallage, and are destitute of olivine, but usually contain much zoisite and frequently also hornblende. Diorites of similar origin occur, sometimes with a predominance of hornblende as in true amphibolites.

The shales are silicified and intersected by innumerable quartz-veins, in which, or projecting from their walls, are frequently abundant zoisite crystals. Yet the metamorphism has not destroyed the microscopic fossils contained in the strata. But the most remarkable example of metamorphism cited by Mr. Becker is the conversion of these same felspathic sandstones into serpentine—an alteration referred to in Prof. Whitney's description of this region. He asserts that field observation conclusively proves the great mass of the serpentine, estimated altogether at more than 1000 square miles, to have been made out of the sandstones, either immediately or through an intermediate granular rock. Sections are seen where sandstone shades off into serpentine, and areas of highly-inclined sandstones pass along the strike into the same rock. In this conversion, the change begins along the cracks, working toward the centres of the included fragments, and producing a structure like that seen in decomposing olivine. The felspar fragments are corroded externally, their cracks being irregularly widened and filled with serpentine which sometimes projects as teeth into the clear felspathic mass. Even the quartz-grains have not been able to resist the alteration, but may be seen with their outer parts replaced by serpentine, which likewise penetrates their interior in long slender green needles. Apatite has also been replaced by serpentine, and the same transformation may be surmised in the case of mica and garnet. Mr. Becker states that chemical analyses and microscopic tests demonstrated that the serpentine diffused through the sandstones and forming the massive exposures is all the same mineral. He will no doubt in due time produce the detailed evidence on which his statements are founded. In the meantime he will of course be prepared for much scepticism and even for angry denial of his results. The careful elucidation of the problem he has attacked cannot but be of enormous service in throwing light upon the vexed question of metamorphism. He claims that the rocks which he has been investigating furnish a colossal example of regional metamorphism of which all the successive stages can be studied. Many thousands of square miles of rock have been subjected to such intense lateral compression that they have been utterly shattered, the average size of the unbroken lumps not being greater than that of an egg. In rocks thus crushed warm interstitial water would have potent chemical reactions. Warm basic solutions are believed to have first been produced, and to have converted the sandstones and some of the shales into holocrystalline compounds containing augite and hornblende. Serpentisation is supposed to have followed at a lower temperature, while silicification came last, as the solutions finally became acid. Geologists will await with impatience the appearance of the monograph in which these conclusions are maintained.

THE COMPOSITION OF THE EDIBLE BIRD'S-NEST (COLLOCALIA NIDIFICA)

THE nature of the material which forms the edible bird's-nest has been the subject of some controversy. In 1817 Sir Everard Horne (*Phil. Trans.*, 1817, p. 337) suggested that it is the product of the activity of certain glandular structures which he figures in his paper, and which he associates with the gastric glands. In the "Origin of Species" (6th ed., p. 228) Darwin indorses the view of its being entirely a secretion by the bird, speaking of it as "inspissated saliva," and showing how the amount of saliva devoted to nest construction differs with different species. He mentions in particular a North American species which he says "makes its nest (as I have seen) of sticks agglutinated with saliva and even with flakes of this substance." Writing at about the same time, Bernstein (*Journal für Ornithologie*, 1859, p. 111) connects the nature of the material with certain developments of the bird's salivary glands, which he says are noticeable during the nest-building season.

On the other hand, it has been maintained by many observers that the nest of this species of swift is con-

Fig. I

Celebes nest. Lamellated structure (mag. 50 diam.).

structed similarly to the North American species referred to by Darwin, the chief difference being that instead of sticks the bird uses certain algæ which are found in considerable masses on the walls of the caves which they frequent in the breeding season. Other algæ also have been suggested as those used.

It has further been supposed that the algæ are partially digested before being utilised, and that after regurgitation the material so acted on is worked up into the form of the nest.

A suggestion was made by Mr. E. L. Layard, H.M. Consul in New Caledonia, in a letter to NATURE, September 17, 1884 (p. 82), which seemed to reconcile the conflicting theories. He says that the first quality of nest which is produced early in the breeding season consists entirely of animal secretion, but that later on, if the first nests are destroyed, the birds cannot replace them by this secretion alone, and so use extraneous substances to help in the construction. The second and inferior qualities of nest are so formed differently from the first.

In a paper published in the *Journal of Physiology* (vol. vi. p. 40), I have given the results obtained from an

examination, both microscopic and chemical, of the nests used for soup at the Health Exhibition of 1884. Since then I have had, by the kindness of Mr. W. T. Thiselton Dyer, Director of the Royal Gardens, Kew, the opportunity of examining various specimens of the nests,

Fig. 2
Borneo nest. No alga present (mag. 200 diam.).

obtained from other places than the first-named, and differing from one another in quality.

The results of my experiments on these nests do not modify in any essential particular those which I had obtained before from the first ones examined. After prolonged soaking all alike became gelatinous in texture, and

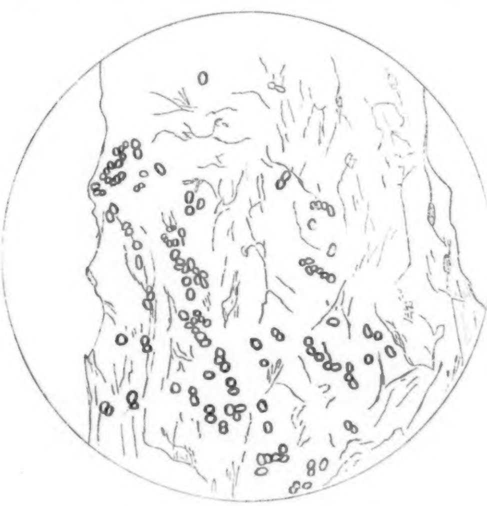

Fig. 3
Celebes nest. Alga present (mag. 200 diam.).

then were easily seen to be made up of laminæ affixed by their faces to each other. Whether the observation was made by teasing with needles or by cutting sections, this laminated structure was very evident. Fig. 1 is a section of a nest from Celebes, enlarged about 50 diameters. The laminæ are fairly regular in disposition, and show no trace of any vegetable structure between them or in their substance. Here and there may be noted small granular bodies resembling epithelium cells. These are seen more distinctly in Fig. 2, which is a section of a nest of inferior quality from Borneo enlarged 200 diameters.

Another nest, also from Celebes, but marked as being of inferior quality showed the presence of algæ. A section of this, taken from the part where the greatest quantity was found, is shown at Fig. 3, the enlargement being again 200 diameters. The presence of the alga in this inferior nest seems at first to bear out Mr. Layard's suggestion, but an examination of the mode in which it is disposed in the nest-substance does not confirm his view of its being here even an agglutination of algæ because the supply of saliva had failed. The alga-cells, though fairly numerous, are not in large quantity when compared with the amount of nest-substance, nor are they regularly placed in layers as would be the case if agglutinated as suggested. Their somewhat scanty amount and their irregular position would be better accounted for on the theory of their being accidental constituents. In many sections debris of one kind or another mixed with the secretion is not at all infrequent, small feathers being the most numerous. In the nest in question the alga was not present throughout, many sections showing none, others a little, the quantity varying very much. The amount found may perhaps be connected with the feeding of the birds, and result from debris of food remaining in the mouth, and so mixed up with the secretion next produced.

The nest-substance gave no micro-chemical reactions that could connect it at all with cellulose, so that it could not be formed by the partial digestion of the alga and regurgitation of the resulting matter. On the other hand, it did give very striking evidence of its close relationship with the body *mucin* described by various authors,[1] and well known as a product of the animal body. The reactions obtained with the first material used (that from the Health Exhibition) were confirmed completely by the experiments made upon the nests from Kew. JOS. R. GREEN

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NOTES

MR. ADAM SEDGWICK, M.A., Fellow and Lecturer of Trinity College, Cambridge, has been nominated by the President and Council of the Royal Society to be recommended for election by the Society on June 4, in place of the late T. R. Lewis, one of the selected candidates, who died soon after the selection was made.

SINCE our last week's note the eruption of Mount Etna has gone on increasing in violence. A correspondent of the *Standard* sends some valuable notes on the progress of the eruption. Under date Catania, May 19, 8.40 a.m., he writes :—"The eruption of Mount Etna assumed alarming proportions last evening at 5 o'clock. Earthquake shocks were felt in all the communes in the immediate vicinity of the volcano. At Zafferato, where the shocks appear to have been attended with upheaval of the soil, the disturbance is described as being so severe that the panic-stricken inhabitants fled from the neighbourhood. In the district of Bronte heavy showers of sand descended, and a gigantic column of thick black smoke was seen to emerge from the central crater of Etna towards the west." "10 a.m. An eruption has occurred near Nicolosi, to the north-west of Montegrosso, quite as severe in character as that of 1883. The lava has begun to stream down towards Nicolosi, accompanied by severe shocks of earthquake. 10.55 a.m. In addition to the

[1] Eichwald, "Ueber das Mucin besonders der Weinbergschnecke," *Annal. Chem. Pharm.*, cxxiv. 1865, pp. 177 to 211. Obolensky, "Ueber Mucin aus der Submaxillardrüse," Pflüger's *Archiv*, vol. iv. p. 336.

eruption which has taken place in Etna itself another one occurred about 2 a.m. in Prince of Naples Mount, situated about 9 kilometres north-east of Nicolosi. This is one of the 'red' mountains, formerly volcanic, but which has hitherto been regarded as exhausted. The lava is running in two streams towards the open country. Repeated and very severe shocks of earthquake are being felt in the vicinity of Etna. 11.55 a.m. A short time ago a severe shock of earthquake of an undulating kind was felt. The eruption of Prince of Naples Mount is increasing in severity. The lava is running rapidly in a double stream towards Nicolosi, to the great danger of the town. Panic amongst the inhabitants." "Nicolosi, May 19, noon. The eruption is assuming terrific proportions. The lava has advanced over 3 kilometres in 8 hours, continuing its course steadily towards Nicolosi. From the central crater, where the eruption is still very active, the lava is flowing towards Montegrosso." "Catania, May 19, 2 p.m. The eruption threatens the destruction of the western portion of the Etna forests. Last night several volcanic rents occurred between Nicolosi and Pedara. May 20. Eleven craters have been opened, of which three have assumed enormous proportions. The lava is advancing rapidly. In some places the stream is 200 metres broad. The central crater, however, emits only vapour and cinders." "Nicolosi, May 20. Three of the craters are raging fearfully, emitting huge stones to a considerable height, and the roar and tumult is terrible. The lava is advancing rapidly, but for the present it is uncertain what direction it may take, whether towards Nicolosi or Belpasso. Shocks of earthquake still continue, but they are less severe in character. The craters are situated behind Montezzano, in the valley between that place and Montenero. The people of the district assert that they can remember no eruption so sudden and alarming as this has been. The scene is indescribable. The streams of lava are in some places more than 200 metres in width. The central crater on the summit continues to vomit large columns of smoke; but from this, according to Prof. Silvestri, no danger is to be apprehended." "Acireale, May 20. Last evening, through the night, and this morning, many shocks of earthquake were felt at Piedimonte, Acisantangelo, Linguaglossa, and Acireale." "Catania, May 20. This morning, with a severe shock of earthquake, the volcanic orifice which was so active in 1883 was reopened." "Nicolosi, May 20. The activity of the central crater is increasing. Montegrosso is the principal site of eruptive force. Great alarm exists among the inhabitants, and the houses most likely to be in danger are being evacuated. The local authorities are on the spot." On Sunday the eruption had greatly diminished, but on Monday morning it broke forth with great violence, and a fresh crater sent out a stream of lava 150 metres wide and 23 deep towards Nicolosi. On Monday evening the news was very disquieting. The violence of the eruption was then greatly increasing, and Nicolosi seemed doomed to destruction. The noise at a considerable distance is described as resembling a continuous cannonade.

AT the meeting of the Paris Academy of Sciences on Monday, M. de Lesseps asked for the appointment of a committee to report on the alleged difference of sea-level on the two sides of the Isthmus of Panama. A similar objection to the Suez Canal had, he said, proved unfounded; and if the present objection were also disposed of, no locks would be necessary in the canal. On the motion of Admiral Jurien de la Gravière, who suggested that the tides might be higher on one side of the Isthmus than on the other, the question was referred to the navigation and astronomy sections. Meantime it is stated that the Technical Commission to whom M. de Lesseps had referred certain questions have affirmed unanimously that there is no insurmountable difficulty to the completion of the Canal according to the technical programme adopted by the Company—that is to say, that there will be no necessity to construct the Canal with locks, the level of the two oceans being the same.

THE committee for the celebration of the centenary of Arago will issue shortly an appeal for subscriptions to erect a statue to the celebrated astronomer on the Place St. Jacques. A new boulevard will be opened in the direction of the meridian for connecting Paris and Montsouris Observatories.

A COUNTY Scientific Society for Middlesex has been constituted, to which both ladies and gentlemen are eligible. The Right Hon. Viscount Enfield, Lord-Lieutenant of the county, has consented to be the President of the Society; and the Right Hon. the Earl of Crawford and Balcarres, the Right Hon. the Earl of Aberdeen, the Right Hon. the Marquis of Ripon, Prof. T. H. Huxley, Sir John Lubbock, Prof. W. H. Flower, Sir Frederick Abel, and Dr. Archibald Geikie having intimated to the Provisional Committee their approval of the scheme and their readiness to become Vice-Presidents of the Society, were at the meeting constituting the Society elected to this office. The following gentlemen were elected members of the Council of the Society, the list to be subsequently increased:—W. Lant Carpenter, Herbert Druce, J. N. Dunning, E. Fitch, G. Griffiths, R. B. Hayward, J. Logan Lobley, Rev. Dr. C. McDowall, Wm. Simpson, Rev. Dr. F. A. Walker, Rev. J. Crane Wharton, W. Mattieu Williams. It is intended that the Society shall hold monthly meetings (evening) from about October to May, and field-meetings during the summer months. Names of ladies or gentlemen desirous of joining may be sent to the Hon. Secretary, Mr. Sydney T. Klein, Clarence Lodge, Church Road, Willesden, N.W.

THE eleventh public annual meeting of the Sunday Society was held on May 22. Sir Henry E. Roscoe, M.P., F.R.S., took the chair as President for the year. Mr. Mark H. Judge read the annual statement, pointing out the progress that had been made during the year in accomplishing the objects which the Society has in view. Sir Henry Roscoe, in his Presidential Address, quoted copiously from the utterances of past Presidents and of eminent men in all departments on behalf of the opening of museums, art galleries, and similar institutions on Sunday. Having had the honour, Sir Henry said, to serve on the Royal Commission on Technical Instruction, he had had full opportunity of observing the effects of the Sunday opening of the numerous museums, industrial, artistic, and scientific, which exist in Continental countries, and he stated that after such observations the unanimous conclusion to which the Commissioners arrived was that the influence which the opening of these museums on Sunday exerted, not only upon the industrial progress of those countries, but upon the moral and intellectual condition of their peoples, had been in the highest degree satisfactory. And so forcible was the evidence on this head then brought before them, that the Commissioners placed a distinct recommendation at the close of their Report, to the effect that museums of art and science and technological collections in this country should be opened to the public on Sundays. They point out in their Report that, in respect to museums, the people of this country stand in a position of great disadvantage as compared with Continental nations, and that to the influence of these collections, as regards the direct bearing they have on art and industrial training, is due much of the abundance of art resource so advantageous to many Continental industries and manufactures. Almost all these Continental art galleries and museums are opened to the public freely on Sundays, and Sir Henry had yet to learn that opposition has been anywhere raised to this, or that any objectionable features have there been introduced. Sunday opening in the United Kingdom, so far as it has gone—and that is a long way—has had an effect the reverse of that

anticipated by its opponents: museums, galleries, and libraries have been open on Sundays in thirteen towns for some years, with such success that the only opposition in those towns is one which does not show itself in public. The town of Oldham is now trying the experiment of Sunday opening for four Sundays, to-morrow being the second Sunday; and Sir Henry had a letter from the Mayor of that important northern town, stating that on last Sunday crowds of well-behaved persons passed through the gallery, and that he is satisfied of the importance and good influence of the movement. Sir Henry concluded:—"We challenge the Sabbatarian party to be true to their own opinions, as we are to ours. We ask for more of what we say has been a blessing. They refuse their assent on the plea that Sunday opening *per se* is a curse. They show by their actions that they are only half believers in their own statements. We challenge them to take a vote on the direct question as to whether those museums and galleries now open should remain open or not." The meeting authorised the Committee of the Society to send a memorial to H.R.H. the Prince of Wales, requesting that in the interest of the community the Colonial and Indian Exhibition should be opened on a few Sundays by free tickets.

MR. EDGAR CROOKSHANK writes, with reference to our article on the Royal Society *soirée* last week, to say that by a slip the exhibition of micro-photographs and preparations of Bacteria were assigned to his friend Mr. Cheshire. The collection of photographs was exhibited by request, and represent many months' work in endeavouring to overcome the difficulties of obtaining satisfactory results with the use of high powers (1-25th, 1-18th, and 1-12th, o.i.). In many cases of preparations of Bacteria, such as *cover-glass preparations*, and especially the so-called *impression-preparations*, and particularly where there is much detail, Mr. Crookshank maintains that photography is the only satisfactory means for obtaining an accurate picture. In such cases photography excels in affording us absolute faithfulness.

THE hybrid trout reared at the South Kensington Aquarium from the ova of the sea-trout, *S. trutta*, impregnated with the milt of the common trout, *S. fario*, in December last, show unmistakable signs of hardihood. Comment has been made previously in this journal upon the subject, as the facts adduced evidence the capacity of Salmonidæ to give ova without descending to the sea. There has been a much less percentage of mortality not only amongst the ova but the fry. At the present time they are located in a special pond at the Delaford Park Fishery belonging to the National Fish Culture Association, where they continue to thrive exceedingly well. They were encouraged to feed after losing their sac sooner than other species of fry; and without exception are growing rapidly.

ON Saturday last 12,000 Severn salmon fry were turned into the River Dee. The ova were spawned by the Severn Fishery Board, and hatched out by the National Fish Culture Association. A large number of the same species will be deposited this week in the Severn, having been reared at South Kensington for this purpose.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Capt. Boyle; two Squirrel Monkeys (*Chrysothrix sciurea*) from Brazil, presented by Mr. George Liddell; a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. F. E. A. Prince; three Speckled Terrapins (*Clemmys guttata*), seven Painted Terrapins (*Clemmys picta*), a Sculptured Terrapin (*Clemmys insculpta*), eleven Striped Snakes (*Tropidonotus sirtalis*), three Ribbon Snakes (*Tropidonotus saurita*), seven Dekays Snakes (*Ischnognathus dekayi*), four Grass Snakes (*Cyclophis vernalis*) from North America, presented by Mr. Samuel Garman, C.M.Z.S.; three Spanish Terrapins (*Clemmys lepresa*), European, presented by Mr. Cuthbert Johnson; a Quail (*Coturnix communis*), European, presented by Mr. Kenneth Lawson; four Menobranchs (*Menobranchus lateralis*) from North America, presented by Prof. Ramsay Wright; two Wild Ducks (*Anas boschas*), British, presented by Mr. G. Edison; a Common Viper (*Vipera berus*), British, presented by Mr. Percy E. Coombe; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; two Spotted Hyænas (*Hyæna crocuta*) from South Africa, two Ruffs (*Machetes pugnax*), three Viperine Snakes (*Tropidonotus viperinus*), European, two Lion Marmosets (*Midas rosalia*), a Spotted Cavy (*Cœlogenys paca*), two Ariel Toucans (*Ramphastos ariel*), two Crested Curassows (*Crax alector*), a Zenaida Dove (*Zenaida amabilis*) from Brazil, purchased; four Bernicle Geese (*Bernicla leucopsis*), European, received in exchange; a Gayal (*Bibos frontalis*), an African Wild Ass (*Equus tæniopus*), a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE HELIOMETER OF THE YALE COLLEGE OBSERVATORY.—From Dr. Elkin's report of the work done during the year ending June 1, 1885, we learn that the principal object of research has been the triangulation of the Pleiades, to which work the heliometer was devoted from September 1884 to March 1885. It was originally intended to confine the investigation to the stars measured at Königsberg and to carry out only one method of triangulation. The scheme has been extended, however, to include all the stars in the Bonn *Durchmusterung*, within certain limits, down to magnitude 9·2, making sixty-nine stars in all, and also to obtain a determination of the relative positions of the stars which should be strictly comparable with the Königsberg work, viz. measurement of distances and angles of position of the stars from Alcyone. The observations have all been reduced provisionally; the final reduction cannot be undertaken until the results of the meridian observations of the end stars of two zones serving to determine the scale value and zero of position have been received from the observatories which have consented to make them. Measures of the moon from neighbouring stars have also been made on thirty-six nights near the first and last quarters, and the diameter of the moon has been measured at opposition on seven occasions. Observations have also been made of the diameter of Venus, the outer ring of Saturn, and of Titan referred to its primary. It is now proposed to devote the heliometer to systematic investigations in stellar parallax, and, judging from the results which have been obtained by Gill and Elkin at the Cape, we may expect that most valuable work will be done in this direction with the heliometer at Yale College also.

ECLIPSE OF JUPITER'S FOURTH SATELLITE.—Mr. Marth pointed out some two years ago, in a paper read before the Royal Astronomical Society (*Monthly Notices*, vol. xliv. p. 241), the importance of observing those eclipses of the fourth satellite of Jupiter which commence or end a series, in order to obtain data for correcting the tables. A slight error in latitude shows itself very strikingly on such occasions in the duration of the eclipse. Such observations are unfortunately very rare, the Greenwich eclipse observations from 1836 to 1883 not affording a single instance of an observation which will assist in correcting the latitude. Mr. John Tebbutt, Windsor, New South Wales, noticing that two of the last eclipses of the cycle just closed were visible in New South Wales, drew attention to these facts in the *Sydney Morning Herald* for March 27. We learn from a communication in the number for April 1 of the same paper that Mr. Tebbutt was successful in his own observations of the phenomenon. The disappearance of the satellite took place at 14h. 38m. 34s. Windsor mean time, or 15m. 43s. *before* the time given in the *Nautical Almanac*. The reappearance, on the contrary, was 8m. 55s. *late*, the duration thus being 24½m. longer than the predicted time. Mr. Russell, at Sydney, noted that the satellite had already disappeared when he began to observe, quite six minutes before Mr. Tebbutt lost sight of it. Mr. Tebbutt was convinced, however, of the accuracy of his now observation.

A NEW COMET.—Mr. W. H. Brooks, Red House Observatory, Phelps, New York, discovered a bright comet on May 23, 15h. Greenwich mean time. Its position was as follows:—R.A. 11h. 55m., Decl. 8° 55′ N.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 MAY 30—JUNE 5

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 30

Sun rises, 3h. 52m.; souths, 11h. 57m. 15·7s.; sets, 20h. 2m.; decl. on meridian, 21° 48′ N.: Sidereal Time at Sunset, 12h. 35m.

Moon (New on June 2) rises, 2h. 37m.; souths, 9h. 20m.; sets, 16h. 14m.; decl. on meridian, 7° 36′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 20	10 58	18 36	17 39 N.
Venus	2 22	9 5	15 48	7 43 N.
Mars	11 57	18 35	1 13*	6 44 N.
Jupiter	12 57	19 15	1 33*	2 50 N.
Saturn	5 50	14 1	22 12	22 46 N.

* Indicates that the setting is that of the following morning.

	h.	
May 30	1	Venus in conjunction with and 1° 18′ north of the Moon.
June 4	10	Saturn in conjunction with and 4° 1′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	May 30,	2 57	*m*
			June 3,	2 37	*m*
S Cancri	8 37·4	19 17 N.	May 31,	22 12	*m*
S Bootis	14 19·1	54 20 N.	June 2,		*M*
δ Libræ	14 54·9	8 4 S.	May 30,	1 16	*m*
U Coronæ	15 13·6	32 4 N.	,, 31,	3 34	*m*
R Herculis	16 1·1	18 41 N.	June 2,		*M*
U Ophiuchi	17 10·8	1 20 N.	May 31,	3 2	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·4	27 47 S.	June 5,	2 20	*M*
W Sagittarii	17 57·8	29 35 S.	,, 2,	21 30	*m*
U Sagittarii	18 25·2	19 12 S.	,, 1,	21 40	*M*
β Lyræ	18 45·9	33 14 N.	,, 1,	2 25	*M*

M signifies maximum; *m* minimum.

Meteor Showers

The shortness of the nights at this season of the year greatly interferes with meteor observation, and no great periodical shower occurs at this time. Meteors from the following radiants amongst others have been observed. Near Cor Caroli, R.A. 206°, Decl. 39° N.; near ε Coronæ, R.A. 240°, Decl. 25° N.; near β Lyræ, R.A. 280°, Decl. 29° N.; near κ Cephei, R.A. 289°, Decl. 80° N.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
407 Birmingham	17 14 2	2 15·4 N.	III.
D.M. + 17° 3241	17 20 49	17 1·1 N.	III.
Arg. Oeltzen 17681	18 1 7	21 16·3 S.	Bright lines
D.M. + 43° 2890	18 3 21	43 26·3 N.	III.
458 Birmingham	18 38 51	36 50·7 N.	IV.
464 Birmingham	18 43 43	8 2·0 S.	IV.
δ^2 Lyræ	18 50 30	36 45·2 N.	III.
R Lyræ	18 51 52	43 47·6 N.	III.
222*c* Schjellerup	18 58 18	5 51·2 S.	IV.

GEOGRAPHICAL NOTES

In his presidential address at the annual meeting of the Royal Geographical Society on Monday, the Marquis of Lorne, referring to the matter of geographical education, said that the interest excited in the subject by the Society's recent action has been so great, and the expectation that the Society will continue it by taking some positive ste s towards encouraging improvements in the position of geography in schools and Universities is so general, that the Council have felt encouraged and indeed bou d to carry the scheme further. The Educational Committee of the Society therefore made certain suggestions to the Council, which are now under consideration, and will probably be adopted. The pr ncipal of these suggestions relates to the appointment of a lecturer in geography to deliver courses where the Council may direct. In this matter the Council will take suitable steps to obtain the co-operation of the Universities of Oxford and Cambridge. In order still further to encourage the scientific study of geography at the Universities, the Committee suggest that a prize or travelling scholarship be given every alternate year to a student who has shown marked ability in geographical subjects, and who may desire to visit one of the less-known districts of Europe, or the Mediterranean or Black Sea shores, and any results be communicated to the Society. One or other of the annual grants which are at the Society's disposal might be devoted to this purpose. Another suggestion is aimed at reaching the intelligent middle and working classes through the medium of the University Extension Scheme. For this purpose a small annual grant is proposed. Another is that a medal be given by the Society to the student reported by the examiners to have done best in physical geography in the first part of the Natural Sciences Tripos (Honours Examination). And finally, in order that all classes of schools may be reached, it is proposed that prizes be offered for competence in geography to the students at the various training-colleges. "Here we reach the fountain-head of education, and if we can secure adequate attention to geography in the institutions which send forth yearly troops of teachers to our Board and elementary schools, the Society will have accomplished much. It is evident, then, that the Society has already accomplished a great deal. The mind of the public has been aroused and greatly enlightened on the subject; our best schools and Universities have expressed their willingness to co-operate as far as possible in carrying out improvements; and there can be little doubt that our proposed further action will bring results which the Council and all interested in geography have long desired."

In a paper contributed to the *Bulletin* of the Moscow Society of Naturalists (1885, No. 2) M. Smirnoff continues his most valuable delimitation of the vegetable zones of the Caucasus, which forms an introduction to a flora of the vascular plants of the region. He subdivides Transcaucasia into several zones, the central zone extending east to the meridian of Shemakha. This limit does not correspond to any orographical features, but separates from the remainder of Transcaucasia the region subject to the influence of the Caspian Sea. The littoral of the Black Sea in Transcaucasia is distinguished fro n the rest of the region by high winter temperatures. As far as lat. 44° N., and even at an altitude of 150 metres, the winter is as mild as in Provence or in Central Italy, only the setting in of warm weather in spring being a little later. But as soon as the chain is crossed we find a rapid decrease of the winter temperature, so that Yekaterinodar, on the northern slope of the chain, although only 20 miles further to the north and 1° of longitude more to the east than Novorossiysk, has an average temperature during December and January 4° lower. Baku and Lencoran have winters very much like those of the Venetian littoral, but Derbent shows a sudden decrease of 3°·5 of temperature in January, probably due to the influence of ice gathering in the northern part of the Caspian Sea, while Petrovsk, only 70 miles further north on the same coast, shows a further sudden decrease of temperature in January. The ranges of the monthly average temperatures of different places show a still greater difference of climate. Thus, on the Black Sea coast, south of lat. 44° N., and even at Kutais, the difference between the warmest and the coldest months does not exceed 18° to 19°; it is the same as at Trieste and Athens. But in Ciscaucasia it reaches 25°, and on the Caspian littoral it varies from 22°·4 at Baku to 29°·8 at Petrovsk. It is still greater on the Armenian plateau (30° to 35°); while in Central Transcaucasia it is generally less than 25°, and rapidly diminishes with the altitude of the place, reaching no more than 19°·5 at Shusha. These few data, together with a map of isotherms prepared by M. Smirnoff, give a broad general idea as to the climatic conditions of the Caucasus, and the consequent distribution of different regions of vegetation through the country.

TELESCOPIC OBJECTIVES AND MIRRORS: THEIR PREPARATION AND TESTING[1]

It would probably lend an additional interest to a technical subject such as I have to bring before you to-night, could I preface my description of the processes now employed in the

[1] Lecture given at the Royal Institution on Friday, April 2, 1886, by Mr. Howard Grubb, F.R.S., F.R.A.S.

construction of telescopic objectives by a short historical account of what has been attempted and achieved in the past, but time will not permit.

A very few words, however, on the history of glass manufacture are necessary.

As I pointed out last Saturday afternoon, Dollond's brilliant discovery, which afforded a means of achromatising objectives, rendered possible their construction of greater size and perfection than formerly, provided suitable material could be obtained. But the chromatic errors being removed, faults in the material hitherto masked by them were detected, and it was not until after many years that Guinand, a lowly but gifted Swiss peasant, succeeded in producing glass disks of a considerable size and free from these defects.

The secrets of his process have been handed down in his own family to M. Feil, of Paris (one of his descendants), and also through M. Bontemps, who for a time was associated with Guinand's son, and afterwards accepted an invitation from Messrs. Chance Bros. and Co., of Birmingham, to assist them in an endeavour to improve that branch of their manufacture. Only these two houses, so far as I am aware, have succeeded in manufacturing optical disks of large size.

Testing of Optical Glass.—Let me here say a few words respecting the testing of optical glass; I mean of the material of the glass, quite apart from the optician's work in forming it into an objective. When received from the glass manufacturer it is sometimes in this state, roughly polished on both sides, and sometimes in this, in which as you see there are small windows only, facets as they are called, polished on the edges. In case of lenses for telescopic objectives, it is always well to have them roughly polished on the sides, to avoid the chance of having to throw away a lens after much trouble and labour has been spent on it.

There are only three distinct points to be looked to in the testing of optical glass: (1) general clearness and freedom from air-bubbles, specks, pieces of "dead metal," &c.; (2) homogeneity; (3) annealing.

The first is the least important, and needs no instructions for detection of defects, any one can see these. The second is much more important, and much more difficult to test.

The best test for homogeneity is one somewhat equivalent to Foucault's test for figure of concave mirrors.

The disk of glass should be either ground and polished to form a convex lens, or if that be not convenient, it should be placed in juxtaposition with a convex lens of similar or larger size, and whose excellence has been established by previous experience.

The lens or disk is then placed opposite some small brilliant light, a small gas flame generally suffices, and at such a distance that a conjugate focus is formed at other side and at a convenient distance. When the exact position of this focus is found, the eye is placed as nearly as possible so that the image of flame is formed on the pupil. On looking at it with the eye in this position, the whole lens should appear to be "full of light"; but at the slightest movement to one side the light will disappear and the lens appear quite dark. If the eye be now passed slowly backwards and forwards between the position showing light and darkness, any irregularity of density will be most easily seen.

Of course, like everything else, some experience is necessary.

The rationale of this is very obvious. When the eye is placed exactly at the focus of a perfect lens, the image formed on the pupil is very small, and the slightest movement of the eye will cause the light to appear and disappear. If the eye be not at the focus, the pencil of light will be larger, and consequently it will require a much greater movement of the eye to cause the light to disappear. Now if any portion of the lens be of a different density to the general mass, that portion will have a longer or a shorter focus; consequently while the light flashes off the general area of the lens quickly, it still remains on the defective portions.

By imitating this arrangement and substituting a camera for the eye and forming the focus of a small point of light on the stop of the lens, I have succeeded in photographing veins in glass, and sometimes have found this useful as a record.

The third point—that of proper annealing—is easily tested by the polariscope.

For small disks the usual plan is to hold them between the eye and a polarising plane, such as a piece of glass blackened at back or a japanned surface, and look at them through the facets, using as an analyser a Nicol prism.

Larger sizes, which are polished on the surfaces, can be more easily examined. It is difficult to describe the appearances, but I will put a few disks into the lantern polariscope and endeavour to point out what amount of polarisation may safely be permitted in disks of glass to be used for objectives.

The composition of metallic mirrors of the present day differs very little from that used by Sir Isaac Newton. Many and different alloys have been suggested, some including silver or nickel or arsenic; but there is little doubt that the best alloy, taking all things into account, is made with 4 atoms of copper, and 1 of tin, which gives the following proportions by weight: copper, 252, tin, 117·8.

Calculation of Curves.—Having now obtained the proper material to work upon, the first thing necessary is to calculate the curves to give to the lenses, in order that the objective, when finished, may be of the required focus, and be properly corrected for the chromatic and spherical aberrations.

As this lecture is intended to deal principally with the technical details of the process, I do not intend to occupy your time for more than a few moments on this head, nor indeed is it at all necessary. In my lecture last Saturday I explained the principles of achromatism, and in many published works full and complete particulars are given as to the calculation of the curves—particulars which are sufficient, and more than sufficient, for the purpose.

Much has been discussed and written concerning the calculation of curves of objectives, and much care and thought has been bestowed by mathematicians on this subject, and, so far as the actual constructors are concerned, a certain amount of veil is thrown over this part of the undertaking, as if there were a secret involved, and as if each had discovered some wonderful formulæ by which he was enabled to calculate the curves much more accurately than others.

I am sorry to have to dispel this illusion. Practically the case stands thus. The calculation of the curves which satisfy the conditions of achromatism and desired focus is a most simple one, and can be performed by any one having a very slight algebraical knowledge in a few minutes, provided the refractive indices and dispersive power of the glass be known. Both Messrs. Chance and Feil supply these data quite sufficiently accurately for small-size objectives. Speaking for myself, I am quite content to take the figures as given by these glass manufacturers for any disks up to 10 inches in diameter. If over that size, I grind and polish facets on the disk and measure the refractive and dispersive powers myself.

The calculations of the curves required to satisfy the conditions of spherical aberration are very troublesome, but fortunately these may be generally neglected.

Some years ago the Royal Society commissioned one of its members to draw up tables for the use of opticians, giving the curves required to satisfy the conditions of both corrections for all refractive and dispersive indices.

A considerable amount of labour was expended on this work, but in the end it was abandoned, for it was found that the calculation of these curves was founded on the supposition that all surfaces produced by the opticians were truly spherical; while the fact is, a truly spherical curve is the exception, not the rule. The slightest variation in the form or figure of the curve will produce an enormous variation in the correction for spherical aberration, and it was soon apparent that the final correction for spherical aberration must be left to the optician and not to the mathematician. *Object-glasses cannot be made on paper.* When I tell you that a sensible difference in correction for spherical aberration can be made by half an hour's polishing, corresponding probably to a difference in the first place of decimals in radii of the curves, you will see that it is practically not necessary to enter upon any calculation for spherical aberration. We know about what form gives an approximate correction; we adhere nearly to that, and the rest is done by figuring of the surface.

To illustrate what I mean. I would be quite willing to undertake to alter the curves of the crown or flint lens of any of my objectives by a very large quantity, increasing one and decreasing the other so as to still satisfy the conditions of achromatism, but introducing theoretically a large amount of positive or negative spherical aberration, and yet to make out of the altered lens an object-glass perfectly corrected for spherical aberration.

I am now speaking of ordinary sizes. For very large sizes it is usual to go more closely into the calculations; but I may

remark that it is sometimes possible to make a better objective by deviating from the curves which give a true correction for spherical aberration and correcting that aberration by figuring, rather than if the strictly theoretical curves were adhered to. So far, then, as any calculation is required, the ordinary formulæ given in the text-books may be considered amply sufficient.

Having now determined on the curves, we have to consider the various processes which the glass has to undergo from the time it is received in this form from the glass manufacturer to the time when it is turned out a finished objective.

The work divides itself into five distinct operations: (1) rough grinding; (2) fine grinding; (3) polishing; (4) centering; (5) figuring and testing.

(1) The rough grinding or approximate shaping of the glass is a very simple process. The glass is cemented on a holder, and is held against a revolving tool supplied with sand and water, and of a shape which will tend to abrade whatever portions are necessary to be removed to produce the required curves. These diagrams will illustrate the various operations.

(2) Fine grinding. The tools used for fine grinding are of this form, and are made of either brass or cast iron. I prefer cast iron, except for very small sizes. They are grooved on the face, in the manner suggested by the late Mr. A. Ross, in order to allow the grinding material to properly distribute itself.

If two spherical surfaces be rubbed together they will, as may be supposed, tend to keep spherical; for the spherical is the only curve which is the same radius every part of its surface. If fine dry abrading powder be used between, the same result will be obtained; but, if wet powder be used, the surface will no longer continue spherical, but will abrade away more on the centre and edge than in the zone between. It was to meet this difficulty that the late Mr. A. Ross devised the idea of the distributing grooves. The fine grinding process is the first of the series which calls for any skill on the part of the operator.

That the *modus operandi* of the grinding be the more easily understood, let me explain the principle of the process in a few words.

When two surfaces of unequal hardness are rubbed together with emery powder and water between the two, each little particle of the powder is at any given moment in either of these conditions: (*a*) embedded into the softer surface; (*b*) rolling between the two surfaces; (*c*) sliding between the two surfaces.

Those particles which become embedded in the softer material do no work in abrading it, and but little in abrading the harder. They generally consist of the finer particles, and are kept out of action by the coarser which are rolling or sliding between the surfaces. Further, those that are purely rolling do little or no work. The greater part of the work is performed by those particles which are facetted and which *slide* between the two surfaces.

As the grinder is always of a much softer material than the glass, there is much more friction between the grinder and these particles than between the glass and the same particles, and therefore they partially adhere to the grinder and are carried by it across the face of the glass. This being so, it is now easy to perceive what the best conditions for rapid grinding are. Not too little emery, for then there will not be enough of abrading particles; not too much, for then the particles will roll on each other and tend to crush and disintegrate each other instead of abrading the glass, but just sufficient to form a single layer of particles between the grinder and the glass surface.

In the grinding of the small lenses, I mean up to 5 or 6 inches diameter, it is usual to carry out the entire grinding processes by hand; above that size by machinery. Surfaces up to 12 or even 15 inches can be ground by hand; but the labour becomes severe, and for my part I am gradually reducing the size for which the hand grinding is used, as I find the machine work more constant in its effects.

The machinery used is the same as that employed for the polishing operation, and I shall describe it under that head further on.

In the fine grinding operation by hand, the glass is usually cemented on to a holder of this form, having (for smaller sizes) three pieces of cork, to which the lens is attached, and this holder being screwed to a spud or nose on top of a post screwed to the floor. The operator, having applied the proper quantity of moist emery powder between the grinder and the glass, proceeds to work the former over the latter in a set of peculiar strokes, the amplitude and character of which he varies according to circumstances, at the same time that he changes his position round the post every few seconds. . . .

Although, as I have shown, the harder material is abraded very much more than the softer, yet the softer (the grinder) suffers considerable abrasion as well as the glass, and the skill of the operator is shown by the facility with which he is able to bring the glass to the curve of the grinder without altering the curve or figure of the latter.

It is even possible for a skilled operator to take a lens of one curve and a grinder of, say, a deeper curve, and by manipulation to produce a pair of surfaces fitting together, and of shallower curves than either.

Measurement of the Curves.—In the early stages of grinding, gauges of the proper radius, cut out of sheet brass or sheet steel, are used for roughly testing the curves of the lenses; but when we get to the finer grinding process it is necessary to have something much more accurate.

For this purpose a spherometer is used. It is made in various forms, generally with three legs terminating in three hardened steel points, which lie on the glass, and a central screw with fine thread, the point of which can be brought down to bear on the centre of the glass. In this way the versed sine of the curve for a chord equal to diameter of circle formed by these points is measured, and the radius of curve can be easily calculated from this.

I do not find the points satisfactory for regular work. They are apt to get injured or worn, and for ground surfaces are a little uncertain, as one or other of the feet may find its way into a deep pit. This particular spherometer has three feet, of about half an inch long, which are hardened steel knife-edges forming three portions of an entire circle. In using this it is laid on the surface to be measured, and the screw with micrometer head is turned till the point is felt to touch the surface of glass. This scale and head can then be read off. The screw in this instrument has fifty threads to the inch, and the head is divided into 100 parts, so that each division is equal to $\frac{1}{5000}$ of an inch. With a little practice it is easy to get determinate measures to $\frac{1}{10}$ of this, or $\frac{1}{50000}$ of an inch, and by adopting special precautions even more delicate measures can be taken, as far probably as $\frac{1}{100000}$ or $\frac{1}{150000}$ of an inch, which I have found to be practically the limit of accuracy of mechanical contact.

To give an idea of the delicacy of the instrument, I bring the screw firstly into contact with the glass. Now the screw is in good contact; but there is so much weight still on the three feet, that, if I attempt to turn it round, the friction on the feet oppose me, and it will not stir except I apply such force as will cause the whole instrument to slide bodily on the glass. Now, however, I raise the whole instrument, taking care that my hands touch none of the metal-work, and that the screw be not disturbed. I lay my hands for a moment on part of the glass where centre screw stood, and thus raise its temperature slightly, and on laying the spherometer back in the same place, you now see that it spins on the centre screw, showing how easily it detects what to it is a large lump, caused by expansion of the glass from the momentary contact of my hand.

Flexure.—One of the greatest difficulties to be contended with in the polishing of large lenses is that of flexure during the process.

It may appear strange that in disks of glass of such considerable thickness as are used for objectives, any such difficulty should occur; but a simple experiment will demonstrate the ease with which such pieces of glass can be bent, even under such slight strain as their own weight.

We again take our spherometer and set it upon a polished surface of a disk of glass of about $7\frac{1}{2}$ inches diameter and $\frac{3}{4}$ inch thick. I set the micrometer head as in the former experiment to bear on the glass, but not sufficiently tight to allow the instrument to spin round. This has now been done while the glass, as you see, is supported on three blocks near its periphery. I now place one block under the centre of disk and remove the others thus, and you see the instrument now spins round on centre screw.

It is thus evident that not only is this strong plate of glass bending under its own weight, but it is bending a quantity easily measurable by this instrument, which, as I shall presently show, is quite too coarse to measure such quantities as we have to deal with in figuring objectives.

After this experiment no surprise will be felt when I say that it is necessary to take very special precautions in the supporting of disks during the process of polishing, to prevent danger of

flexure; of course if the disks are polished while in a state of flexure, the resulting surface will not be true when the cause of flexure is removed.

For small-sized lenses no very special precautions are necessary, but for all sizes over 4 inches in diameter I use the equilibrated levers devised by my father, and utilised for the first time on a large scale in supporting the 6-foot mirror of Lord Rosse's telescope. These have been elsewhere frequently described, but I have a small set here as an example.

I have also sometimes polished lenses while floating on mercury. This gives a very beautiful support, but it is not so convenient, as it is difficult to keep the disk sufficiently steady while the polishing operation is in progress without introducing other chances of strain.

So far I have spoken of strain or flexure during the process of working the surface; but even if the surface be finished absolutely perfectly, it is evident from the experiment I showed you that very large lenses when placed in their cells must suffer considerable flexure from their own weight alone, as they cannot then be supported anywhere except round the edge.

To meet this I proposed many years ago to have the means of hermetically sealing the tube, and introducing air at slight pressure to form an elastic support for the objective, the pressure to be regulated by an automatic arrangement according to the altitude. My attention was directed to this matter very pointedly a few years ago from being obliged to use for the Vienna 27-inch objective a crown lens which was, according to ordinary rules, much too thin.

I had waited some years for this disk, and none thicker could be obtained at the time. This disk was very pure and homogeneous, but so thin that, if offered to me in the first instance, I would certainly have rejected it. Great care was taken to avoid flexure in the working, but, to my great surprise, I found no difficulty whatever with it in this respect. This led me to investigate the matter, with the following curious results.

If we call f the flexure for any given thickness t, and f' the flexure for any other thickness t', then $\frac{f}{f'} = \frac{t^3}{t'^2}$ for any given load or weight approximately. But as the weight increases directly as the thickness, the flexure of the disks due to their own weight, which is what we want to know, may be expressed as $\frac{f}{f'} = \frac{t}{t'}$.

Let us now consider the effect of this flexure on the image. In any lens bent by its own weight, whatever part of its surface is made more or less convex or concave by the bending has a corresponding part bent in the opposite direction on the other surface, which tends to correct the error produced by the first surface. This is one reason why reflectors which have not this second correcting surface are so much more liable to show strain than refractors. If the lens were infinitely thin, moderate flexure would have no effect on the image. The effect increases directly as the thickness. If then the flexure, as I have shown, decreases directly as the thickness, and the effect of that flexure increases directly as the thickness, it is clear that the effect of flexure of any lens due to its own weight will be the same for all thicknesses; in other words no advantage is gained by additional thickness.

This has reference, of course, only to flexure of the lens in its cell after it has been duly perfected, and has nothing to do with the extra difficulty of supporting a thin lens during the grinding and polishing processes.

Polishing.—The polishing process can be, and is often, conducted precisely in the same manner as the grinding, except that the abrading powders used (oxide of iron, rouge, an oxide of tin, putty-powder) is of a finer and softer description, and the surface of the polishing tool is made of a softer material than the metallic grinder.

Very nearly all my polishing is done on the machine I shall presently describe; but before doing so, I will, with your permission, say a few words on the general principles of the polishing process. Various substances are used for the face of the polisher—fine cloth, satin or paper, and pitch. Pitch possesses two important qualities which render it peculiarly suitable for this work, and it is a curious fact that we owe its application for this purpose to the extraordinary perspicuity of Sir Isaac Newton, who we may fairly say was the first to produce an optically perfect surface, and that that material is not only still used for the purpose, but is, as far as I know, the only substance which possesses the peculiar qualifications necessary to fulfil the required conditions. With skill and care, moderately good surfaces can be obtained from cloth polishers; but it is easy to see why they can never be perfect. There is a certain amount of elasticity in cloth and in its "nap," and there is consequently a tendency to round off the surfaces of the pits left by the grinding powder, and to polish the bottom or floor of these pits at the same time as the upper surface. It is easy to show mathematically that any process which abrades the floors of the pits at the same time as general surfaces even in a very much less degree, can never produce more than an approximation to a perfect surface, and practice agrees with the theory. Paper is said to be much used by the French opticians. I can say nothing about it. I have tried it and failed to produce a perfect surface with it, nor indeed should I expect it. It is of course open to the same objection as cloth. Pitch possesses, as I said, two most important qualities which render it suitable for the work; the first, in its almost perfect inelasticity; the second, a curious quality of subsidence, which we utilise in the process.

If we watch with a microscope, or even a magnifier, the character of two surfaces during the process of polishing, the one with cloth, and the other with pitch, the difference is very striking. With the cloth polisher, the polish appears much quicker, and it would at first sight appear as if the same polishing powder abraded more quickly on the cloth than on the pitch polisher, but I do not believe that such is the case, for if we look at the surface with a magnifier we shall find that, while all the surface has assumed a polished appearance, the surface itself has retained some of the form of the original pitted character with the edges rounded off; while in the pitch half-polished surfaces the floors of the pits are as gray as ever, and the edges are sharp and decisive. In pitch polishing, too, a decided amount of polish appears very quickly, and then for many hours there appears to be little or no further effect. Suddenly, however, the remaining grayness disappears, and the surface is polished. The reason of this is very obvious. The polisher being very inelastic, polishes first only the tops of the hills, and has to abrade away all the material of which these hills are composed before it reaches the valleys or floors of the pits. When it does reach them, the proper polish quickly appears. The second quality of pitch, that of subsidence, is also most valuable.

Pitch can be rendered very hard by continued boiling. By pitch I mean the natural bituminous deposit which comes to us from Archangel, not gas-tar pitch. It can be made so hard that it is impossible to make any impression on it with the finger-nail without splitting it into pieces; and yet even in this hard condition, if laid on an uneven surface it will in a few days, weeks, or months subside and take the form of whatever it is resting upon. The cohesion of its particles is not sufficient to enable it to retain its form under the action of gravity; and as this condition is that which science tells us marks the difference between solids and liquids, we must, paradoxical though it may appear, class even the hardest pitch among liquid instead of solid substances.

Now how do we utilise this peculiar quality?

The polishing tool is made by overlaying a metal or wooden disk formed to nearly the required curves by a set of squares of pitch, and while these are still warm pressing them against the glass, the form of which they immediately take.

In the grinding process I showed you that the regulation of the abrasion was managed partly by the character of the stroke given, and partly by the local touches given to the tool by the stoning process.

In polishing we still retain the same facilities for modifying the stroke, and the same rules I gave apply generally to the polishing process as well as the grinding; but we have not got any process equivalent to that of the local stoning, and even if we had it would be useless, for this very quality of subsidence of the pitch would in a few minutes cause any part of its surface which had been reduced to come into good contact again; we must therefore look for some other means for producing more or less abrasion whenever we require it. This we effect by modifying the size of the squares of pitch in the various zones. Practically it is done in this way by a knife and mallet. Whenever the squares are reduced, the abrasion will be less.

This is a well-known method of regulation; but the rationale is, I think, not generally understood. It is generally explained that there is less abrasion because there is less abrading surface. I do not think this is the true, or at least the entire, explanation. In order to understand the action, you must conceive the pitch to be constantly in a state of subsidence, the amount of that

subsidence depending of course on the pressure placed upon it. Now, if we reduce the size of the squares in any zone while retaining the same distance from centre to centre of squares, we increase at first the pressure per unit of area on the pitch squares in that zone, and consequently the subsidence will be greater, and the action will not be so tight or severe on that zone.

I know of no substance but pitch and a few of the resins which possesses this peculiar quality except perhaps ice, and it is curious to think that the same quality which in ice allows of that gradual creeping and subsidence and consequent formation of glaciers with their characteristic moraines, &c., will in pitch help us to produce accurate optical surfaces.

Polishing-Machines.—The two best-known polishing-machines are those of the late Earl of Rosse and the late Mr. Lassell, the general forms of which were shown in these diagrams. Time will not permit me to enter into a minute description of their working, nor is it necessary, as both have been often described.

A few words, however, as to the different character of the strokes given by these machines may be interesting. The stroke of Lord Rosse's machine may be imitated in hand-work by the operator traversing the polisher or mirror in a series of nearly straight strokes, of about one-third the diameter of the glass, to and from himself, at the same time that he keeps walking slowly round the post, and instead of allowing the centre of polisher to pass directly over the centre of mirror, each stroke that he gives he slides a little (about one-tenth diameter) to one side and then a little to the other.

Mr. Lassell's stroke may be imitated by causing the polisher to describe a series of nearly circular strokes a little out of the centre, walking round the post at the same time; thus the centre of polisher will describe a series of epicycloidal or hypocycloidal curves on the speculum.

Many years ago my father devised a machine, figured and described in Nichols's "Physical Science," by which either of these motions could be obtained. He appeared to have got better results with Mr. Lassell's strokes, for he afterwards devised a machine which gave the same character of stroke, but over which the operator had greater control, and this machine has been used for many years with great success. Like all machines, however, which give a series of strokes constantly recurring of the same amplitude, it is apt to polish in rings It is impossible to obtain absolute homogeneity in the pitch patches, and if any one square be a shade harder than the general number, and that square ends its journey at each stroke at the same distance from the centre of speculum or glass, there will almost surely be a change of curvature in that zone. To avoid this I have made a slight modification in the machine, which has increased its efficiency to a great extent. I will now place in the lantern a model of this machine, and first draw you a few curves with the machine in its old state, and afterwards in its improved state.

In order to convey some idea of the relative quantities of material removed by the various processes, I have placed upon the walls a diagram which will illustrate this point in two distinct ways.

The diagram itself represents a section of a lens of about 8 inches aperture and 1 inch thick, magnified 100 times, and shows the relative thickness of material abraded by the four processes.

The quantity removed by the rough grinding process is represented on this diagram by a band 25 inches wide, the fine grinding by one $\frac{8}{10}$ inch wide, the polishing by a line $\frac{1}{50}$ inch wide, while the quantity removed by the figuring process cannot be shown even on this scale, as it would be represented by a line only $\frac{1}{10000}$ inch thick.

I have also marked on this diagram the approximate cost of abrasion of a gramme of material by each of the four processes, viz. :—

		£	s.	d.	
Rough grinding,	about	0	0	1	per gramme.
Fine grinding,	,,	0	0	7½	,,
Polishing,	,,	0	10	0	,,
Figuring,	,,	48	0	0	,,

Figuring and Testing.—By the figuring process I mean the process of correcting local errors in the surfaces, and the bringing of the surfaces to that form, whatever it may be, which will cause the rays falling on any part to be refracted in the right direction. When an objective has undergone all the processes I have described, and many more which are not so important, and with which I have not had time to deal, and when the objective is centred and placed in its cell, it is, to look at, as perfect as it will ever be, but to look through and use as an objective it may be useless. The fact is that when an objective has gone through all the processes described, and is in appearance a finished instrument, I look upon it as about one-fourth finished. Three-fourths of the work has probably to be done yet. True, sometimes this is by no means the case, and I have had instances of objectives which were perfect on the first trial; but this is, I am sorry to say, the exception and not the rule.

This part of the process naturally divides itself into two distinct heads :—

(1) The detection and localisation of faults—what may, in fact, be termed the diagnosis of the objective.

(2) The altering of the figures of the different surfaces to cure these faults. This may be called the remedial part.

It may be well here to try to convey some idea of the quantities we have to deal with, otherwise I may be misunderstood in talking of great and small errors.

I have before mentioned that it is possible to measure with the spherometer quantities not exceeding $\frac{1}{50000}$ of an inch, or with special precaution much less even than that; but useful as this instrument is for giving us information as to the general curves of the surface, it is utterly useless in the figuring process; that is, an error which would be beyond the power of the spherometer to detect, would make all the difference between a good and a bad objective.

Take actual numbers and this will be evident. Take the case of a 27-inch objective of 34 feet focus; say there is an error in centre of one surface of about 6 inches diameter, which causes the focus of that part to be $\frac{1}{10}$ of an inch shorter than the rest.

For simplicity's sake, say that its surface is generally flat; the centre 6 inches of the surface therefore, instead of being flat, must be convex and of over 1,000,000 inches radius. The versed sine of this curve, as measured by spherometer, would be only about $\frac{1}{250000}$, 4 millionths of an inch, a quantity mechanically unmeasurable, in my opinion.

If that error was spread over 3 inches only instead of 6 inches, the versed sine would only be about $\frac{1}{1000000}$. Probably the effect on the image of this 3-inch portion of $\frac{1}{10}$ inch shorter focus would not be appreciable on account of the slight vergency of the rays, but a similar error near edge of objective certainly would be appreciable. Until, therefore, some means be devised of measuring with certainty quantities of 1 millionth of an inch and less, it is useless to hope for any help from mechanical measurement in this part of the process.

If, then, no known mechanical arrangement be delicate enough to measure these quantities, how, it may be asked, are these errors detected?

The answer to this is, that certain optical arrangements enable us to carry our investigations far beyond the limits of mechanical accuracy. Trials of the objective or mirror as a telescope are really the crucial test, but there are various devices by which defects are detected and localised.

The best object to employ is generally a star of the third or fourth magnitude, when such is available, but as it frequently occurs that no such object is visible, recourse is had to artificial objects. The minute image of the sun reflected from little polished balls of speculum metal, or even a thermometer bulb is a very good object; polished balls of black glass are also used with good effect; but as the sun also is of somewhat fickle disposition in this country, we have frequently to have recourse to artificial light. Small electric lamps, such as this, with their light condensed and thrown on a polished ball are very useful. In fact, I am never without one of them in working order.

For the detection and localisation of errors it is very useful to be provided with sets of diaphragms which leave exposed various zones of the surface, the foci of which can then be separately measured, but a really experienced eye does not need them.

For concave surfaces, Foucault's test is useful. I shall not trespass on your time to explain this in detail, as it is described very fully in many works, in none better than in Dr. Draper's account of the working of his own reflecting telescope. This diagram will give an idea of the principle of the system, which is really the same as what I have described as useful for detecting want of homogeneity in the substance of the glass.

This system is extremely useful for concave spherical surfaces, but is not available for convex surfaces, and only partially available for concave parabolic surfaces.

The really crucial test is, as I said before, the performance of

the objective when used as a telescope; and the appearance of the image not only at the focus, but on each side of it, conveys to the practised eye all the information required for the detection of the errors.

If an objective have but one single fault, its detection is easy; but it generally happens that there are many faults superposed, so to speak. There may be faults of achromatism, and faults of figure in one or all the surfaces; faults of adjustment, and perhaps want of symmetry from some strain or flexure; and the skill of the artist is often severely taxed to distinguish one fault from the rest and localise it properly, particularly if, as is generally the case, there be also disturbances in the atmosphere itself, which mask the faults in the objective, and permit of their detection only by long and weary watching for favourable moments of observation.

It would be impossible in one or a dozen of such lectures as this to enumerate all the various devices that are practised for the localisation of errors, but a few may be mentioned, some of which have never before been made public.

For detection of faults of symmetry, it is usual to revolve one lens on another and watch the image. In this way it can generally be ascertained whether it is in the flint or crown lens.

With some kinds of glass the curves necessary for satisfying the conditions of achromatism and spherical aberration are such that the crown becomes an equi-convex and the flint a nearly plano-concave of same radius on inside curve as either side of the crown. This form is a most convenient one for the localisation of surface errors in this manner.

The lenses are first placed in juxtaposition and tested. Certain faults of figure are detected. Now calling the surfaces A B C D in the order in which the rays pass through them, place them again together with Canada balsam or castor-oil between the surfaces B and C, forming what is called a cemented objective. If the fault be in either A or D surface, no improvement is seen; if in B or C, the fault will be much reduced or modified. Now reverse the crown lens, cementing surfaces A and C together. If same fault still shows, it must be in either B or D. If it does not show, it will be in either A or C. From these two experiments the fault can be localised.

It often happens that a slight error is suspected, but its amount is so slight that it appears problematical whether an alteration would really improve matters or not. Or the observer may not be able to make up his mind as to the exact position of the zone he suspects to be too high or too low, and he fears to go to work and perhaps do harm to an objective on which he has spent months of labour, and which is almost perfect. In many such cases I have wished for some means by which I could temporarily alter the surface and see it so altered before actually proceeding to abrade and perhaps spoil it.

During my trials with the great objective of Vienna, I thought of a very simple expedient, which effects this without any chance at all of injuring the surface. If I suspect a certain zone of an objective is too low, and that the surface might be improved by lowering the rest of it, I simply pass my hand, which is always warmer than the glass, some six or eight times round that particular zone. The effect of this in raising the surface is immediately apparent, and is generally too much at first, but the observer at the eye end can then quietly watch the image as the effect goes off, and very often most useful information is thus obtained. The reverse operation, that of lowering any required part of the surface, is equally simple. I take a bottle of sulphuric ether and a camel's-hair brush, and pass the brush two or three times round the part to be lowered, blowing on it slightly at the same time; the effect is immediately perceived, and can always be overdone if required.

So far then for the diagnosis. Now for the remedy. When the fault has been localised, the lens is again put upon the machine and the polisher applied as before, the stroke of the machine and the size of the pitch patches being so arranged as to produce, or tend to produce, a slightly greater action on those parts that have been found to be too high (as before described while treating of the polishing processes).

The regulation of the stroke, excentricity, speed, and general action of the machine, as well as the size and proportion of the pitch squares, and the duration of the period during which the action is to be continued, are all matters the correct determination of which depends upon the skill and experience of the operator, and concerning which it would be impossible to formulate any very definite rules. All thanks are due to the late Lord Rosse and Mr. Lassell, and also to Dr. de la Rue, for having published all particulars of the process which they found capable of communication; but it is a notable fact that, as far as it is possible to ascertain, every one who has succeeded in this line has done so, not by following written or communicated instructions, but by striking out a new line for himself; and I think I am correct in saying that there is hardly to be found any case of a person attaining notable success in the art of figuring optical surfaces by rigidly following directions or instructions given or bequeathed by others.

There is one process of figuring which is said to be used with success among Continental workers. I refer to the method called the process of local touch. In this process those parts, and those parts only, which are found to be high, are acted upon by a small polisher.

This action is of course much more severe; and if only it were possible to know exactly what was required, it ought to be much quicker; but I have found it a very dangerous process. I have sometimes succeeded in removing a large lump or ring in this way (by large I mean 3 or 4 millionths of an inch), but I have also and much oftener succeeded in spoiling a surface by its use. I look upon the method of local touch as useful in removing gross quantities, but for the final perfecting of the surface I would not think of employing it.

In small-sized objectives the remedial process is the most troublesome, but in large-sized objectives the diagnosis becomes much the more difficult, partly on account of the rare occurrence of a sufficiently steady atmosphere. In working at the Vienna objective it often happened when the figure was nearly perfect that it was dangerous to carry on the polishing process for more than ten minutes between each trial, and we had then sometimes a week to wait before the atmosphere was steady enough to allow of an observation sufficiently critical to determine whether that ten minutes' working had done harm or good. It must not be supposed either that the process is one in which improvement follows improvement step by step till all is finished. On the contrary, sometimes everything goes well for two or three weeks, and then from some unknown cause, a hard patch of pitch perhaps or sudden change of temperature, everything goes wrong. At each step, instead of improvement there is disimprovement, and in a few days the work of weeks or months perhaps is all undone. Truly any one who attempts to figure an objective requires to have the gift of patience highly developed.

In view of the extraordinary difficulty in the diagnostic part of the process with large objectives, it is my intention to make provision which I hope may reduce the trouble in the working of the new 28-inch objective for the Royal Observatory, Greenwich.

Two of the greatest difficulties we have to contend with are: (1) the want of homogeneity in the atmosphere, through which we have to look in trials of the objective, due to varying hygrometric and thermometric states of various portions; and (2) sudden changes of temperature in the polishing-room. The polisher must always be made of a hardness corresponding to the existing temperature. It takes about a day to form a polisher of large size, and if before the next day the temperature changes 10° or 15°, as it often does, that polisher is useless, and a new one has to be made, and perhaps before it is completed another change of temperature occurs. To grapple with these two difficulties I propose to have the polishing-chamber under ground, and, leading from it, a long tunnel formed of highly glazed sewer-pipes about 350 feet long, at the end of which is placed an artificial star illuminated by electric light; on the other side of the polishing-chamber is a shorter tunnel, forming the tube of the telescope, terminating in a small chamber for eye-pieces and observer. About half-way in the long tunnel there will be a branch pipe connected to the air-shaft of the fan, which is used regularly for blowing the blacksmith's fire, and through this, when desired, a current of air can be sent to "wash it out" and mix up all currents of varying temperature and density. It may be found necessary even to keep this going during observations.

By this arrangement I hope to be able to have trials whenever required, instead of having to wait hours and days for a favourable moment.

Figuring of Plane Mirrors.—There is a general idea that the working of a plane mirror or one of very long radius is a more difficult operation than those of more ordinary radii. This is not exactly the case. There is no greater difficulty in figuring a low curve than a deep one, but the difficulty in the case of absolutely plane mirrors consists simply in the fact that in their figuring there

is one additional condition to be fulfilled, viz. that the general radius of curvature must be made accurate within very narrow limits. In figuring a plane mirror to use, for instance, in front of even a small objective, say 4-inch aperture, an error in radius which would cause a difference of focus of $\frac{1}{100}$ of an inch would seriously injure the performance. This would be about equivalent to saying that the radius of curvature of the mirror was about 8 miles, the versed sine of which, with the 6-inch spherometer, would be about $\frac{1}{80000}$ of an inch. Now what I mean to convey is this: that it would be just as difficult to figure a convex or concave lens of moderate curvature as a flat lens of the same size if it were necessary to keep the radius accurate to that same limit, *i.e.* one-tenth of a division of this spherometer.

Lick Observatory.—For the final testing of large objectives or mirrors, it is necessary to have them properly mounted, and in such a manner that they can be directed conveniently on any celestial object, and kept so directed by clockwork, to enable the observer to devote his whole attention to the testing. I had not intended touching at all on the subject of the mounting of telescopes, but I have been asked to call your attention to this model of a proposed observatory for Mount Hamilton, California, as it embraces some novel features, but I shall do so in only a very few words.

Most here are probably aware that a monster observatory is in course of erection in California, a large sum of money having been left for the purpose by a Mr. Lick. The observatory is already partly complete, and contains some excellent instruments of moderate size, the work already done with which warrants us to hope that the great 36 inch refractive about to be erected will be placed under more favourable conditions for work than any other large telescope in the world.

The 36-inch objective is at present in process of construction by the Messrs. Clark of America, but the mounting has not yet been contracted for.

Some years since, in a paper published in the *Transactions* of the Royal Dublin Society, I shadowed forth a principle which I considered should be adopted in great telescopes of the future. The trustees of the Lick Observatory having invited me to design an instrument for the 36-inch objective, I have put into practical form what I had before given but general principles of, and the design which this model illustrates is the result.

Whether this design will ever be carried out or not I cannot tell, but even as a proposal I trust it may be interesting enough to excuse my introducing it (somewhat irrelevantly perhaps) to your notice to-night.

The design includes the equatorial itself, with its observatory, dome, and provision for enabling the observer to reach the eye end conveniently.

The conditions I laid down for myself in designing this observatory were that it would be possible for the observer single-handed to enter the equatorial room at any time, and that, without using more physical exertion than is necessary for working the smallest-sized telescope, or even a table-microscope, he should be able to open the 70-foot dome, turn it round backwards and forwards, point the equatorial to any part of the heavens, revolving it in right ascension and declination to any extent, and finally (the most difficult of all) to bring his own person into a convenient position for observing. I say this last is the most difficult of all, for I think any who have worked with larger instruments will allow that there is generally far more trouble in moving the observatory chair (so called) and placing it in proper position than in pointing the instrument itself. In this instrument the "chair" would require to be 25 feet high, and with its movable platform, ladder, balance-weight, &c., would weigh probably some tons. Even if very perfect arrangements were made for the working of this chair, the mere fact that the observer, while attempting to make the most delicate observations, is perched upon a small and very unprotected platform 25 feet above the floor, and in perfect darkness, tends to reduce his value as an observer to an extent only to be appreciated by those who have tried it.

No matter how enthusiastic a man may be at his work, I would not put a high value on his determinations if made while in a position which calls for constant anxiety for his own personal safety. I would go even further still, and say that even personal comforts or discomforts have much to do with the value of observations.

I propose, therefore, that all the various motions should be effect d by water-power. There are water engines of various forms now made, some of which have no dead point, and having little *vis inertia*, are easily stopped and started, and are consequently well adapted for this work.

I propose to use four of them: one for the right ascension motion of the instrument, and one for the declination; one for revolving the dome, and one for raising and lowering the observer himself; but instead of having anything of the nature of a 25-foot chair or scaffold, I propose to make the 70-foot floor of the observatory movable. It is balanced by counterpoise weights, and raised and lowered at will by the observer. Then the observer can without any effort raise and lower the whole floor, carrying himself and twenty people if desired, to whatever height is most convenient for observation; and wherever he is observing, he is conscious that he has a 70-foot floor to walk about on, which even in perfect darkness he can do in safety.

The valves and reversing gear of the water engines are actuated by a piece of mechanism, the motive power of which may be a heavy weight raised into position some time during the previous day by man- or water-power. By means of a simple electrical contrivance, this piece of machinery itself is under the complete control of the observer, in whatever part of the room he may be, and he carries with him a commutator of a compact and convenient form, with eight keys in four pairs, each pair giving forward and backward movements respectively to (A) telescope movement in right ascension; (B) telescope movement in declination; (C) revolution of dome; (D) raising of floor.

The remaining operation—opening of shutter—is easily effected without any additional complication.

It is only necessary to anchor the shutter (which moves back horizontally) to a hook in the wall and move the dome in the opposite direction by motion C; the shutter must, of course, be opened by this motion. . . .

It is very possible that there may be some here who have found what I have had to say on the subject of the figuring of objectives very unsatisfactory. They may have expected, naturally enough, that, instead of treating of generalities to such a large extent as I have done, I should have given precise directions, by the following of which rigidly any person could make a telescopic objective.

To those, however, who have followed me in my remarks, the answer to this will probably have already suggested itself. It is the same answer which I give to those who visit my works and ask what the secrets of the process are, or if I am not afraid that visitors will pick up my secrets. All the various processes which I have described up to that of the figuring are, I consider, purely mechanical processes, the various details of which can be communicated or described as any mechanical process can be; but in the last final and most important process of all there is something more than this. A person might spend a year or two in optical works where large objectives are made, and might watch narrowly every action that was taken, see every part of the process, take notes, and so forth, and yet he could no more expect to figure an objective himself than a person could expect to be able to paint a picture because he had been sitting in an artist's studio for the same time watching him at his work. Experience, and experience only, can teach any one the art, and even then it is only some persons who seem to possess the power of acquiring it.

A well-known and experienced amateur in this work declared his conviction that no one could learn the process under nine years' hard work, and I am inclined to think his estimate was not an exaggerated one.

True, it may be said that large objectives can be and are generally turned out by machinery, but what kind of an objective would any machine turn out if left to guide itself, or left to inexperienced hands?

At the risk of being accused of working by what is generally called the rule of thumb, I confess that conditions often arise, to meet which I seem to know intuitively what ought to be done, what crank to lengthen, what tempering is required of the pitch square; and yet if I were asked I should find it very hard to give a reason for my so doing which would even satisfy myself.

I may safely say that I have never finished any objective over 10 inches diameter, in the working of which I did not meet with some new experience, some new set of conditions which I had not met with before, and which had then to be met by special and newly-devised arrangements.

A well-known English astronomer once told me that he considered a large objective, when finished, as much a work of art as a fine painting.

I have myself always looked upon it less as a mechanical

operation than a work of art. It is, moreover, an art most difficult to communicate. It is only to be acquired by some persons, and that after years of toilsome effort, and even the most experienced find it impossible to reduce their method to any fixed rules or formulæ.

INDIAN CASTINGS AT THE INDIAN AND COLONIAL EXHIBITION

AT the last meeting of the Iron and Steel Institute Mr. C. Purdon Clarke, C.I.E., Keeper of the Indian Section, South Kensington Museum, read a paper "On Certain Descriptions of Indian Castings" as follows:—

The importation of partly manufactured material is at present exercising considerable influence over many of the native arts of Oriental countries and India. The supply of machine-made thread has doubled the village hand-looms in some districts of Madras, and gold thread from Germany has enabled the brocade weavers to compete with the imitation brocades sent in from Europe.

In some handicrafts, however, the supply of European material has produced a contrary effect. Iron and steel, bar and rod, have displaced an ancient industry, and sheet copper and brass have robbed the founder of half his work. Formerly the only means of producing sheet-metal was by hammering cast plates, an expensive method, only resorted to when thin flat coverings were required for wooden or other objects. For very large vessels, where weight was required to be kept down and strength maintained, hammered sheet was used; but generally the founder was employed, to save as much as possible the labour of forming the furnished castings which required but little beating out, trimming, and brazing.

In the case of a bowl, or flat jar with a narrow mouth, the founder would prepare a cast not unlike in shape and thickness that of an ordinary flower-pot saucer, from which, by constant hammering, the bulbous sides would be formed, projecting beyond the rim, which would remain of its first diameter and thickness. When finished, such a vessel would be nearly double the size of the first cast, and a remarkable example of the native knowledge of the composition of bronzes and annealing processes.

It is worthy of noting that the chief means of detecting modern from old Persian and Saracenic metal vessels is by examining the brazen joints, which in ancient vessels are rare. When not found, a close examination will show the vessel to be a thin casting, the ornamentation being by inlay, or chasing and hammering, which, being done after the cast is made, gives the reverse side the appearance of chased sheet metal.

So far as he could ascertain, there were three methods of casting practised in India. The first, by moulds in sand; the second, moulds in clay not unlike plasterers' piece-moulds; the third, clay moulds formed on a wax model, the *cire perdu* of Europe.

The first of these was well known in Europe, but the second was, he believed, now described for the first time. In preparing the mould, impressions of the various parts of the pattern are taken in clay, and these pieces when nearly dry are, after trimming, stuck neatly together, and kept in place by several layers of mud, in which some fibre is mixed. The mould when ready has but one vent, which, placed on the most convenient side, is carried up into a sort of bottle-neck. If the object is small, several moulds are attached together, and the vents united by a single short neck of clay, to which a crucible, inclosed in an egg-shaped ball of clay, is attached. The size of this crucible depends upon the exact amount of metal required to fill the mould or moulds; and this quantity being known by experience, the founder places it inside before closing up. No provision is made for the escape of air from the mould when the metal is poured in. The mould and crucible (now in one piece) is allowed to dry; and after several coats of clay, tempered with fibre, have also been well baked on by the sun, the furnace is prepared. This is simply a circular chamber about 2 feet 6 inches in diameter, 2 feet in height, with a perforated hearth and no chimney. Half filled with charcoal, a good heat is obtained by the use of several sheepskin bellows from beneath. When ready, as many moulds as the furnace will hold are placed in it, the crucible end of each being embedded in the fire. A cover is placed over, and the fire kept up until, upon examination, the moulds are found to be red hot. They are then taken, one at a time, and replaced in a reverse position, the crucibles being now above. The metal flows down into a red-hot mould and penetrates the finest portions of the surface without suffering from air or chilling. The fire is allowed to gradually cool, and when the objects are broken out of their clay covering, the metal is soft and malleable.

The third manner of casting (that by the use of a wax pattern which is destroyed in the moulding) was well known, but in one particular case the process had been carried further than would be at first believed, and of this he would now attempt a description.

The object produced is an anklet, a flexible ring about 4 inches in diameter, made from an endless curb chain. Such curb chain trinkets are common in India, and are generally made from thick silver wire rings interlinked and soldered one by one. In this example the anklet is of bronze, and consists of a complicated chain of forty-three detailed links, the whole being cast by a single operation. The first part of the process is the preparation of a pattern in wax, a delicate work, each link having to pass through four others, and to bear three small knobs or rosettes. These are in two instances but ornaments; the third, however, serves as a channel for the metal to enter each ring.

Then commences the most difficult part of the work, each ring having to be slightly separated, and this is effected by painting in a thin coat of fine clay until there is sufficient to form a partition. Other coats of clay are added until a thickness of about half an inch is attained, when a groove is cut round the upper side of the ring, and deepened until the row of knobs is bared. The wax is then melted out, and the mould attached to a crucible as before described. When cast, and the mould broken away, the chain comes out inflexible, being attached to a rod which runs round where the groove was cut. This is broken off, and the chain is complete.

Having been consulted respecting the trades to be represented in the Indian Courts of the Colonial and Indian Exhibition, he recommended amongst others a good brassfounder to be sent. Dr. Tyler, who was charged with the collection of these artisans, engaged one of the best he could find, but up to the present the foundry is not in working order.

One of these combined crucible moulds was submitted for inspection, with fragments of another, also a cast curb chain anklet; the author concluding by thanking the members for this opportunity of publishing an interesting process.

A NEW SPECTROMETER

IN equipping the Physical Laboratory of University College, Dundee, I felt considerable difficulty in deciding on a spectrometer for accurate work; it was easy to get a simple instrument for qualitative experiments and rough quantitative work, but it was only after consulting several friends and communicating with two or three firms that about two years ago I wrote to Mr. Hilger, in the hope that from him we might obtain an instrument capable of working to as high a degree of accuracy as would enable our best students in the laboratory to do advanced work. Considering that a second of arc is by no means an unusual limit of error in angular measurement, and that it is of the order 1 : 1,000,000, the whole circle being unit, we thought that while further capability in reading power would be more than counterbalanced by various indeterminate errors, yet it should be possible to obtain this accuracy with a suitable instrument.

Prof. Liveing was kind enough to give us valuable information about one of his own instruments, of which the plans were sent to us by Mr. Hilger for inspection; and Mr. Capstick and I finally decided to ask Mr. Hilger whether he could not arrange two microscopes on the instrument in place of the one which Prof. Liveing's has.

As a consequence, Mr. Hilger presented suggestions for a spectrometer which is now in this college, and is capable of reading directly to one second of arc and yielding reliable results. Its construction is very simple. The collimator stands on a heavy pillar by itself; and the circle, divided to five minutes of arc on a ring 15 inches in diameter, with six radial spokes, is carried on another pillar. The telescope, counterpoised, turns on the same axis, but does not touch the circle at any point; and the reading is managed as follows: from the telescope-bearing a double girder with a semicircular plan tied across its diameter by tubes of brass stretches horizontally above the semi-circumference of the divided circle; to this girder are fixed, at its ends,

two long focus microscopes, whose axes produced intersect the divided circle at the extremities of a diameter. They are read by means of a pointer and spider-line micrometer, whose head is divided into 300 parts, each of which represents one second of arc. The microscopes are carried at such a height that they easily pass the collimator, and they can be read in any position, and the light from the collimator passes clear under the girder.

J. E. A. STEGGALL

THE ABACUS IN EUROPE AND THE EAST

AT a late meeting of the Asiatic Society of Japan (reported in the *Japan Mail*), Dr. Knott read a paper on "The Abacus, and its Scientific and Historic Import." A portion of the paper described the various arithmetical processes connected with the *soroban*, the form of the abacus employed in Japan. The writer pointed out that in all arithmetical operations up to the extraction of the cube root, the *soroban* really possessed distinct advantages over ordinary ciphering. This in itself explained why the instrument, which in Europe is suggestive of an infant school, has in the East survived till the present day. The rest of the paper was a discussion of the peculiar position which the abacus, used in its widest signification, holds in the history of the progress of arithmetic and mathematics, and of science and civilisation generally. The following is an abstract of the argument, the ultimate object of which was to explain why the abacus had died in Europe but lived in China, and why the cipher system of numerals had grown up in India but not in China.

The abacus, as used in China and Japan, bears, on the surface of it, evidence of a foreign origin. The numbers are set down on it with the larger denomination to the left—a method which could hardly be believed to have been invented by the Chinese, who tend to work from right to left, and who have always named their compound numbers beginning with the higher denominations. The Chinaman says "one hundred forty-five," as the Englishman does; but the Englishman once said "one hundred five-and-forty," as the German still does; while in some of the Aryan languages of India, and in the Arabic of to-day, the number is named "five-and forty and one hundred." The Arab writes from right to left, so that, had the abacus been invented by such a people who, so to speak, both wrote and spoke inversely, it would have indicated the number as it does. In fact, the abacus could only have arisen in its present form amongst a people who either wrote and spoke directly, or wrote and spoke inversely. As a matter of history, the geographical home of the abacus is India, but, unless there is conclusive evidence to the contrary, we cannot regard it as an invention of Aryan Indians, who, although they wrote directly, spoke inversely. They probably borrowed it from the Semitic merchants, and these, with their inverse speaking and inverse writing, may have invented it, or perhaps received it from a direct-speaking, direct-writing people, such as the highly-cultured Accadians seem to have been. The abacus was evolved, no doubt, from the human hand, which, with its ten fingers, was the only counting-board used by primitive man. Its course of development is quite distinct from that of the symbolic representation of numbers. These latter we can trace through four stages, which may be called the pictorial, the symbolic, the decimal, and the cipher. The pictorial we find in the Egyptian hieroglyphic and the Accadian cuneiform; the symbolic in the hieratic, Phœnician, Hebrew, Greek, Roman, and the host of systems which grew up with the development and spread of alphabets and syllabaries, and the decimal in the simplification of these which live to-day in the Chinese and Tamilic systems. Once the decimal stage was reached, its general similarity to the abacus indications suggested bringing them into still closer correspondence. This took place amongst the Aryan Indians, who, along with their brethren of the West, very soon discarded the abacus for the, to them, more convenient cipher notation. With the Chinese and Tamils, however, no advance was made in this direction, a fact especially surprising in the case of the latter, who have lived in close contact with peoples that have long used the cipher system of numerals. One reason for the Chinese conservatism in so adhering to an unwieldy notation might be their vertical mode of writing, with which no very striking similarity between their symbolising of numbers and the abacus columns would appear. But this does not explain the conservatism of the Tamils, who write from left to right, nor yet the persistence of the abacus for centuries face to face with the Indian cipher system. The explanation is rather to be found in the system of nomenclature, which, being direct both among the Chinese and the Tamils, fitted perfectly with the abacus indications. For this reason the manipulation of the abacus in China and Japan is more rapid and certain than ciphering, and hence, there being no advantage for simple arithmetical operations in the latter, the cipher system did not develop in these countries, and even when introduced from the West in all its vigour could not displace "the rod and the beads." An Aryan Indian, with his inverse-speaking, could never work the abacus with the same facility as the Japanese unless he worked from right to left, a mode of procedure quite foreign to his nature. It is not so foreign to the Chinese and Japanese, however, to work from left to right, as shown in the formation of each individual ideograph employed in writing. Hence the abacus suited the Chinese language better than it did any of the Aryan languages in their original mode of number-naming. The influence of the notation which was developed from Semitic sources under the influence of the abacus, has in later times compelled many of the Aryan languages to assimilate as far as possible to the direct mode of numeration; but in the English *fifteen*, the German *fünfzehn*, and the French *quinze*, we still have the relics of the original inverse mode of naming, alike peculiar to Aryan and Semitic peoples.

In the course of the discussion which followed, it was mentioned that Chinese mathematics were first studied in Japan about 900 A.D., and that the Japanese ascend by powers of 10,000 in their treatment of larger numbers.

THE GAZETTEER OF RUSSIA[1]

WE have received the concluding fascicule of the "Geographical and Statistical Dictionary of the Russian Empire," published by the Russian Geographical Society, and edited by M. P. Semenoff. This monumental work, which was begun more than twenty years ago, has been now concluded in five large octavo volumes, and will remain for many years the most trustworthy and complete source of information for the geography of the empire, exclusive of Poland, but inclusive of the former Russian dominions in America. It may be regretted that the editor of the "Dictionary" has been diverted by so great a variety of geographical, statistical, and administrative work from this undertaking, and that therefore the last fascicule appearing twenty-three years after the first, the statistical information contained in the first fascicules and volumes has become out of date. But notwithstanding that, the "Dictionary" has not become old. Its value is not in the statistical data it contains; it is much more in the excellent geographical descriptions of the localities treated—that is, of each separate government of Russia, Siberia, Turkestan, and Caucasus—of the seas that border Russia, and their islands. Several articles are excellent and most complete monographs, and we need only mention those on the Amur, Caucasus, Sakhalin, the Northern Ocean, or Turkestan to remind geographers of these excellent descriptions of whole regions. The geology, the flora and fauna of each region have received much attention. These descriptions will not soon be old—they can be only completed.

At the end of each article there is, moreover, a most complete bibliography of the larger geographical works in which the place described in the article has been mentioned, as also of monographs dealing with it, and of newspaper articles. This bibliography is invaluable for the geographer. On the whole, the equally high standard of all geographical descriptions and the unity of conception in all of them—the whole being the work of the editor himself, assisted only by M. Zverinsky and very few occasional contributors—make this "Dictionary" occupy one of the first ranks among like publications. An appendix is promised, which will contain descriptions of such regions as the Thian-Shan, Ferganah, and Transbaikalia, which were much explored during the publication of the "Dictionary." They will embody all recent information.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CONVOCATION of the University of London met on Tuesday evening to consider the report of a Special Committee which proposed several important changes in the constitution of the

[1] "Geographitchesko-Statistitcheskiy Slovar Rossiyskoy Imperii," P. P. Semenova.

University. After considerable discussion, resolutions were passed approving of the admission of certain educational institutions having one, or more than one, faculty of University rank as constituent Colleges of the University, of the establishment of a Council of Education, and of certain changes in the constitution of the Senate.

SCIENTIFIC SERIALS

THE most important paper in the *Journal of Botany* for April is the commencement of a Synopsis of the Rhizocarpeæ, by Mr. J. G. Baker, another of the series of this writer's exhaustive monographs of the families of Vascular Cryptogams outside the Ferns. The present instalment includes the genus Salvinia, in which three new species are described, and a portion of Azolla. In the May number we find a continuation of Mr. W. B. Grove's paper on new and noteworthy fungi, in which several new species are described, and one new genus of Sphæroideæ, Collonema. Mr. W. H. Beeby gives further particulars respecting the distribution of his newly discovered *Sparganium neglectum*, and Mr. Arthur Bennett an account of the distribution in Britain of the various species of Potamogeton, in addition to those contained in the second edition of "Topographical Botany."

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 20.—"Relation of 'Transfer-Resistance' to the Molecular Weight and Chemical Composition of Electrolytes." By G. Gore, LL.D., F.R.S.

In the full paper the author first describes the method he employed for measuring the "resistance," and then gives the numerical results of the measurements in the form of a series of tables.

He took a number of groups of chemically related acids and salts of considerable degrees of purity, all of them in the proportions of their chemical equivalent weights, and dissolved in equal and sufficient quantities of distilled water to form quite dilute solutions. The number of solutions was about seventy, and included those of hydriodic, hydrobromic, hydrochloric, hydrofluoric, nitric, and sulphuric acids; the iodides, bromides, chlorides, fluorides, hydrates, carbonates, nitrates, and sulphates, of ammonium, cæsium, rubidium, potassium, sodium, and lithium; the chlorides, hydrates, and nitrates, of barium, strontium, and calcium; and a series of stronger solutions, of equivalent strength to each other, of the chlorides of hydrogen, ammonium, rubidium, potassium, sodium, lithium, barium, strontium, and calcium. A series of similar liquids to those of one of the groups of acids, of equal (not of equivalent) strength to each other, was also included.

As electrodes, he employed pairs of plates of zinc, cadmium, lead, tin, iron, nickel, copper, silver, gold, palladium, and platinum; and separate ones formed of small bars of iridium.

He took each group of solutions, and measured in each liquid separately, at atmospheric temperature, the "total resistance" at the two electrodes, and the separate "resistances" at the anode and cathode respectively with each other, and thus obtained about seventy different tables, each containing about thirty-six measurements, including the amounts of "total," "anode," and "cathode" resistance of each metal, and the "averages" of these for all the metals.

By comparing the numbers thus obtained, and by general logical analysis of the whole of the results, he has arrived at various conclusions, of which the following are the most important:—The phenomenon of "transfer-resistance" appears to be *a new physical relation of the atomic weights*, attended by inseparable electrolytic and other concomitants (one of which is liberation of heat, *Phil. Mag.*, 1886, vol. xxi. p. 130). In the chemical groups of substances examined *it varied inversely as the atomic weights of the constituents, both electro-positive and electro-negative, of the electrolyte, independently of all other circumstances;* and in consequence of being largely diminished by corrosion of the electrodes, it appeared to be intimately related to "surface-tension." He suggests that corrosion may be a consequence, and not the cause of small "transfer-resistance." The strongest evidence of the existence of the above general law was obtained with liquids and electrodes with which there was the least corrosion and the least formation of films; those liquids were dilute alkali-chlorides, with electrodes of platinum.

This research is an extension of a former one on "Transfer-Resistance in Electrolytic and Voltaic Cells," communicated to the Royal Society, March 2, 1885. Further evidence on the same subject has been published by the author in the *Philosophical Magazine*, 1886, vol. xxi. pp. 130, 145, 249.

"A Study of the Thermal Properties of Ethyl Oxide." By William Ramsay, Ph.D., and Sydney Young, D.Sc.

A year ago a paper was communicated to the Society on the behaviour of ethyl alcohol when heated. A similar study of the properties of ether has been made, in which numerical values have been obtained exhibiting the expansion of the liquid, the pressure of the vapour, and the compressibility of the substance in the gaseous and liquid conditions; and from these results, the densities of the saturated vapour and the heats of vaporisation have been deduced. The temperature range of these observations is from $-18°$ to $223°$ C.

It is the authors' intention to consider in full the relations of the properties of alcohol and ether; in the meantime it may be stated that the saturated vapour of ether, like that of alcohol, possesses an abnormal density, increasing with rise of temperature and corresponding rise of pressure; that at 0° the vapour-density is still abnormal, but appears to be approaching a normal state; and that the apparent critical temperature of ether is 194° C.; the critical pressure very nearly 27,060 mm. = 35·61 atmospheres; and the volume of 1 gramme of the substance at 184° between 3·60 and 4 c.c.

Mathematical Society, May 13.—J. W. L. Glaisher, F.R.S., President, in the chair.—Mr. F. W. Watkin was admitted into the Society.—The following communications were made:—On Cremonian congruences contained in linear complexes, by Dr. Hirst, F.R.S.—Solution of the cubic and biquadratic equation by means of Weierstrass's elliptic functions, by Prof. Greenhill.—On the complex of lines which meet a unicursal quartic curve, by Prof. Cayley, F.R.S.—On Airy's solution of the equations of equilibrium of an isotropic elastic solid under conservative forces, by W. J. Ibbetson.—Conic note, by H. M. Taylor.—On the converse of stereographic projection and on contangential and coaxal spherical circles, by H. M. Jeffery, F.R.S.

Zoological Society, May 18.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. C. W. Rosset exhibited a series of photographs taken during his recent visit to the Maldive Islands, and made some remarks on the zoological collections obtained during his expedition.—Mr. Philip Crowley, F.Z.S., exhibited some pupæ of nocturnal Lepidoptera which had been sent to him from Natal; and read some notes from his correspondent, which proved that they were subterranean.—Mr. Joseph Whitaker, F.Z.S., exhibited a specimen of Wilson's Phalarope, said to have been obtained at Sutton Ambian, near Market Bosworth, in Leicestershire.—A communication was read from Dr. A. B. Meyer, C.M.Z.S., containing an account of the known specimens of King William the Third's Bird of Paradise (*Rhipidornis guglielmi-tertii*), and remarking on a fourth specimen which had been recently obtained by the Dresden Museum.—Mr. Frank E. Beddard read a paper on some new or little-known Earthworms, together with an account of the variations in structure exhibited by *Perionyx excavatus*.—Mr. Sclater read a paper on the species of Wild Goats and their distribution. Mr. Sclater recognised ten species of the genus Capra, distributed over an area extending from Spain to Southern India, and from Central Siberia to Abyssinia.

Royal Meteorological Society, May 19.—Mr. W. Ellis, F.R.A.S., President, in the chair.—Mr. L. T. Cave and Rev. C. Malden, M.A., were elected Fellows of the Society.—The following papers were read:—The severe weather of the past winter, 1885–86, by Mr. C. Harding, F.R.Met.Soc. The author showed that the whole winter was one of exceptional cold, not so much on account of any extremely low temperatures experienced, but more from the long period of frost and the persistency with which low temperature continued. In the South-West of England there was not a single week from the commencement of October to March 21 in which the temperature did not fall to the freezing-point. In many parts of the British Islands frost occurred in the shade on upwards of 60 nights between the beginning of January and the middle of March, and during the long frost which commenced in the middle of February and continued until March 17 the temperature fell below the freezing-point in many places on more than 30 consecutive nights. At Great Berkhamsted, in Hertfordshire, frost

occurred on the grass on 73 consecutive nights from January 5 to March 18. The winter of 1885-86 was the only one in which there was skating on the water of the London Skating Club, in Regent's Park, in each of the four months December to March, since the formation of the Club in 1830, and there are but four records of skating in March during the 56 years, and none so long as in the present year. With regard to the temperature of the water of the Thames at Deptford, it was shown that the total range from January 8 to March 20 was only 6°, whilst from March 1 to 19 the highest temperature was 36°·5, and the lowest 35°. The temperature of the soil at the depth of 1 foot was generally only about 2° in excess of the air over the whole of England, and from March 1 to 17 the earth was colder than usual by amounts varying from 6°·3 at Lowestoft to 8°·5 at Norwood. The facts brought together showed that the recent winter was one of the longest experienced for many years, and that in numerous ways it may be characterised as "most severe."—Description of an altazimuth anemometer for recording the vertical angle as well as the horizontal direction and force of the wind, by Mr. L. M. Casella. The author describes an anemometer he has made which records continuously on one sheet the pressure, direction, and inclination of the wind.—Earth temperatures, 1881-85, by Mr. W. Marriott, F.R.Met.Soc. This is a discussion of the observations of the temperature of the soil at various depths below the surface, which have been regularly made at 9 a.m. at several of the stations of the Royal Meteorological Society during the past five years. The results show that the temperature of the soil at 1 foot at nearly all the stations in the winter months is almost the same as that of the air, while in the other months of the year the temperature of the soil is higher than that of the air at all except that of the London stations.—Note on the after-glows of 1883-84, by Mr. A. W. Clayden, M.A., F.R.Met.Soc. The author suggests that the after-glows were the result of the water-vapour erupted from Krakatão, and that the dust and other ejecta played but a secondary part in the production of the phenomena.

SYDNEY

Linnean Society of New South Wales, March 31.—Mr. William A. Haswell, M.A., B.Sc., in the chair.—The following papers were read:—On certain Geckos in the Queensland Museum, by Charles W. de Vis, M.A. A new species of the very curious genus *Nephrurus* is described under the specific name of *lævis*, from its smooth lepidosis, as compared with the only other species, *N. asper*. A species of *Diplodactylus* (*D. venicauda*) is also described. Both lizards are from Northern Queensland.—Description of a new aphanipterous insect from New South Wales, by A. Sidney Olliff, F.E.S., Assistant Zoologist, Australian Museum. The remarkable parasite here characterised under the name *Echidnophaga ambulans* was found in large numbers on the head and breast of a porcupine anteater (*Echidna hystrix*). It differs from the *Pulex echidnæ* described by Denny from the same host in habit as well as in several important points of structure, and is, therefore, regarded as forming the type of a new genus. Unlike the majority of its allies this species does not appear to possess the power of jumping.—On a microscopic fungus parasitic on the Cucurbitaceæ, by E. Haviland, F.L.S. In this paper the author gives an account of his inquiry as to the origin of a disease which has caused much destruction to melon and pumpkin plants during the last three months, and which he has identified as the micro-fungus *Oidium monilioides*. As a preventative he suggests greater care in cultivation, and quotes various authors proving that old plants will thereby be sufficiently vigorous to resist the attacks of the fungus.—Jottings from the Biological Laboratory of Sydney University, by William A. Haswell, M.A., B.Sc., Lecturer on Zoology and Comparative Anatomy. On the myology of the flying squirrel (*Petaurista taguanoides*). In its muscular anatomy the flying phalanger nearly resembles the vulpine phalanger and the *Cuscus*, with a few special modifications, of which the chief is the presence of a peculiar "long femoro-caudal muscle."—Insects of the Fly River, New Guinea, "Coleoptera," by William Macleay, F.L.S., &c. This is the second paper communicated by Mr. Macleay on the insects collected during the recent expedition organised by the Geographical Society of Australia for the exploration of the interior of British New Guinea. The previous paper dealt with the Coleoptera up to the end of the Heteromera. The present one deals with the families *Curculionidæ*, *Brenthidæ*, *Anthribidæ*, and *Longicornia*, comprising in all 96 species, of which 31 are now described for the first time.—The Mollusca of the Pareora and Oamaru systems of New Zealand, by Capt. F. W. Hutton, Hon. Member Linnean Society, New South Wales. Capt. Hutton's paper is a contribution towards the correlation of the Tertiary rocks of Australia with those of New Zealand, and it enumerates 268 species of Mollusca from the Pareora and Oamaru systems, which are probably of Miocene and Oligocene age, of which 184 species are confined to the Pareora beds, 33 species to the Oamaru, while 51 species, of which a few are doubtful, are common to both.

PARIS

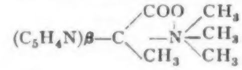
Academy of Sciences, May 17.—M. Jurien de la Gravière, President, in the chair.—Presidential allocution on the occasion of the homage offered to M. Chevreul at the meeting of Monday, May 17, when that illustrious member and *doyen* of the Academy completed his hundredth year. In reply, M. Chevreul assured the audience that to be told his long career had been useful to science and his country was the greatest eulogium he had ever ambitioned.—Observations in reference to the quantitative analysis of the ammonia found in the ground: a reply to M. Schlœsing, by MM. Berthelot and André. The authors point out that the note recently published by them in the *Comptes rendus* was not intended to raise any discussion on M. Schlœsing's theories regarding the absorption of atmospheric ammonia by arable lands. Their main object was to explain a special precaution and a common source of error in the quantitative analysis of the ammonia present in the ground. Nor did they wish to deny that the ground receives in a general way a supply of ammonia from the atmosphere, although they did not consider that this fact had been fully demonstrated by M. Schlœsing's experiments.—Reply to M. Taurines's recent observations on the communication of March 23, 1885, regarding marine engines and the experiments made on board the *Primauguet*, by M. A. Ledieu. The author maintains the general correctness of his conclusions, which are unfairly stigmatised by M. Taurines as "theories conceived *a priori* and at times dangerous."—Remarks on the third volume of the Scientific Mission to Cape Horn, presented to the Academy by M. Mascart. This volume contains all the observations regarding terrestrial magnetism, and MM. Müntz and Aubin's analyses of the specimens of atmospheric air collected by Dr. Hyades. The researches on terrestrial magnetism were greatly aided by a continuous registering apparatus, which was set up by MM. Payen and Le Cannellier, and which worked satisfactorily the whole time the Mission remained in Orange Bay. Incidental reference was made to the subsequent death of M. Payen in France, and of M. Martial, commander of the Expedition, in China.—Elements of the orbit of Brooks's comet, No. 1, by M. Lebeuf. These elements, deduced from observations made at Kiel on April 30, and at Paris on May 4 and 8, are as under:—

T = 1886 June 7·5158 Paris Mean Time.

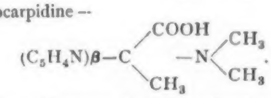
$$\left.\begin{aligned} \Omega &= 193^\circ\ 1'\ 29''\!\cdot\!5 \\ \varpi &= 33\ \ 42\ \ \ 7\cdot1 \\ i &= 87\ \ 47\ \ 34\cdot7 \end{aligned}\right\}\ \text{Mean Eq. } 1886\cdot0.$$
$$\log q = 9\cdot439104$$

—Observations of Brooks's comets made at the Observatory of Algiers with the 0·50 m. telescope, by M. Rambaud.—Measurement of the electric conductivity of the dissolved chloride of potassium, by M. E. Bouty. Between the temperatures of 0° C. and 30° C. the resistance of the solutions of the chloride of potassium is expressed with sufficient accuracy by the binomial formula $r_t = \dfrac{r_0}{1 + at}$. A table is given of the absolute values of the specific resistance r_0 and the relative values of the molecular resistance ρ_0, as well as the values of α.—On the atomic volume of oxygen, by M. E. H. Amagat. M. Wroblewski having recently announced that the atomic volume of oxygen was considerably below 16, the author points out that he had arrived at the same conclusion early in 1885. In his communication of March 2 of that year he stated that under a pressure exceeding 4000 atmospheres he had succeeded in obtaining oxygen with a density higher than 1·25 and at a temperature of 17°.—Observations on the deviation from the vertical on the south coast of France, by M. Germain. From four determinations obtained at Nice, Saint-Raphael, Toulon, and Marseilles, the author infers that on this seaboard the continent attracts the vertical, that is to say, repels the astronomic as opposed to the geodetic zenith, and that this attraction

appears to be exercised by a point situated to the north of Nice in the Alps.—On the barometric pressure of May 13, 1886, when at 4 o'clock in the morning the barometer fell to 737·37 mm., the lowest recorded in Paris since the year 1757, by M. E. Renou. This remarkable fall coincides with violent atmospheric disturbances in Madrid and other parts of Spain, in England and the United States. The stormy weather reached Italy and Germany on May 14, when the Jura and Chaux-de-Fonds were covered with snow.—Action of vanadic acid on the ammoniacal salts (continued), by M. A. Ditte. In this paper the author deals with a second group comprising the sulphate, chromate, iodate, borate, acetate, vanadate, perchlorate, carbonate, and hydrochlorate of ammonia.—On several double silicates of alumina, and of potassa or soda, by M. Alex. Gorgeu. The kaolin with which these silicates are obtained is that used at the Sevres works. This composition when dried at a temperature of 120° C. is almost exactly that of the silicate of hydrated alumina, $2SiO_2Al_2O_3,2HO$. Its action is described on the alkaline haloid salts, on the alkaline carbonates, and on the fused caustic alkalies.—On the combinations of the chloride of zinc with water, by M. R. Engel. Besides that discovered by M. Schindler, the author describes three other hydrates of the chloride of zinc, of which two may be obtained in large isolated crystals.—On a combination of phosphuretted hydrogen with the hydrate of chloral, by M. J. de Girard.—On pilocarpine, by MM. E. Hardy and G. Calmels. For this substance the authors have established the formula—



and for pilocarpidine—



—Researches on the composition of carotine, its chemical function, and its formula, by M. A. Arnaud. This is a carburet of hydrogen ($C_{26}H_{38}$) identical with the orange-red crystallised substance which the author has extracted from the leaves of various kinds of plants. This colouring-matter exists also in a great many fruits, and especially in the tomato, and may in fact be said to be universally present in the roots, leaves, and fruits of plants. It oxidises in the air even at the ordinary temperature, and especially about 70° C., and in solution this oxidation becomes extremely rapid.—Remarks on the bilobites, by M. Stan. Meunier. The author makes a fresh study of these interesting vestiges, without deciding the question whether they are mere animal footprints, as supposed by M. Nathorst, or real fossil algæ, as maintained by MM. Delgado and De Saporta.—Characteristics of the stem of Poroxylon (fossil gymnosperms of the Carboniferous epoch), by MM. C. Eg. Bertrand and B. Renault.—Account of a meteor recently observed on board the steamer *Algérie* in the Gulf of Smyrna, by M. L. Aubouy.

BERLIN

Physiological Society, April 30.—Dr. Wolffberg spoke on the Young-Helmholtz theory of the colour-sense, which he extended in the direction of assuming the existence of red-sensitive, green- and violet-sensitive ganglia in the central organ of sight-perception in the sphere of vision. These ganglia were connected with the red nerves, the green nerves, and the violet nerves, and by means of such nerves communicated with the retina. Seeing, however, that yellow, blue, and white were likewise psychically simple sensations, Dr. Wolffberg assumed specific ganglia for these as well, which, however, stood in connection with the red, green, and violet ganglia, the yellow ganglia being situated at an equal remove from the red and green, but at a further remove from the violet ganglia. Similar was his conception of the situation and connection of the blue and white ganglia. Regarding the sensation of black, he would speak in an address in the immediate future.—Dr. Uhthoff made further communications respecting the dependence of visual sharpness on the intensity of illumination. After an historical survey of the older experiments to determine the relation of visual sharpness to light intensity, he described the results of his own labours in this field. In the case of white light, he had communicated the relation on a former occasion (NATURE, vol. xxxi. p. 476). In the case of yellow light, the visual sharpness under low intensities increased just as rapidly with increasing intensity of light as in the case of white light. The curve, however, in the former case attained a greater height than it did with white, and then likewise proceeded parallel to the abscissa. With red light, on the other hand, the curve kept below the height reached with white light; it rose slower, moreover, and never became parallel. The curve of visual sharpness for green light lay still deeper than for red, and also rose persistently, though slowly. The curve for blue light lay deepest of all, and very soon became parallel to the abscissa of the light intensity. In the case of a green-blind person, the curves for white, yellow, and red were the same as in the case of the normal eye, as there was likewise a coincidence for blue. The curve for green fell almost coincident with the low curve for blue.

BOOKS AND PAMPHLETS RECEIVED

"Contra-Indications for Visiting the High Altitudes," with a Description of the Environs of Maloja, by Dr. A. T. Wise (Churchill).—"The Pictorial Arts of Japan," part ii., by Wm. Anderson (S. Low).—"Bees and Bee-keeping," part ix., by F. R. Cheshire (U. Gill).—"Fancy Pigeons," 3rd edition, part ix., by J. C. Lyell (U. Gill).—"British Cage Birds," part ix., by R. L. Wallace (U. Gill).—"Bicycles and Tricycles of the Year 1886," by H. H. Griffin (U. Gill).—"Mineralogical Magazine," March.—"Journal of Physiology," April.—"Proceedings of the Physical Society, St. Petersburg," vol. xviii. part 4.—"Bulletin de l'Académie Impériale des Sciences de St. Petersburg," vol. xxxi. No. 1.—"Chemical Atlas," part 1, by C. Peddie (Thin, Edinburgh).—"The Baths, Bathing, and Attractions of Aix-les-Bains," by Dr. W. Wakefield (S. Low).—"Bulletin of the United States Fish-Commission," vol. v., for 1885 (Washington).—"Causeries Scientifiques," by Hy. Vivarez (J. Michelet, Paris).—"Proceedings of the American Philosophical Society," April.—"American *versus* English Methods of Bridge Designing" (Japan Mail).—"Third Report on the Chemical Composition and Physical Properties of American Cereals, Wheat, Oats, Barley, and Rye," by C. Richardson (Washington).—"Memoir of Arnold Guyot, 1807 to 1884," by J. D. Dana.

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